London School of Economics and Political Science

Essays in Finance

Ali Choubdaran Varnosfaderani

Thesis submitted to the Department of Finance of the London School of Economics and Political Science for the degree of *Doctor of Philosophy*

September 2025

To my father and mother,
for their endless love and sacrifice
and to my brothers,
my companions through every journey

Declaration

I certify that the thesis I have presented for examination for the Ph.D. degree of the London School of Economics and Political Science is solely my own work.

I declare that my thesis consists of approximately 19,002 words.

Acknowledgements

I am deeply grateful to my supervisor Dirk Jenter for his unwavering encouragement and inspiration throughout this journey. The countless occasions on which I have sought his guidance have been instrumental in bringing this dissertation to completion and in fostering my growth as a researcher.

I would like to extend my heartfelt thanks to the distinguished faculty members at the LSE, including Michela Verardo, Mike Burkart, Dimitri Vayanos, Ulf Axelson, Christopher Polk, Dirk Jenter, Igor Makarov, Ashwini Agrawal, Georgy Chabakauri, Cameron Peng, Kim Fe Cramer, Daniel Ferreira, Juanita Gonzalez Uribe, Peter Kondor, Martin Oehmke, Daniel Paravisini, Liliana Varela, Kathy Yuan, Hongda Zhong, Walker Ray, Ian Martin, Cynthia Balloch, and Jean Pierre Zigrand. I have benefited immensely from their exceptional teaching, cutting-edge research, and invaluable feedback during seminars. I am also deeply grateful to the administrative staff, particularly Mary Comben, for their dedicated support and assistance.

My experience at the LSE would have been immeasurably different without the camaraderie of my fellow doctoral students. I extend my sincere appreciation to Mark Morley, Nicolas Garrido Sureda, Christopher Greiner, Magnus Hjortfors Irie, Jiahong Shi, Zheyuan Yang, Amirabas Salarkia, Bijan Aghdasi, Javad Shamsi, Vahid Rostamkhani, Marc Gischer, Elizaveta Smorodenkova, Davide La Cara, Otavio Bitu, Artemii Andrianov, Oliver Ashtari Tafti, Fernando Soares, and Niels Wagner for their generous help, intellectual companionship, and enduring friendship.

I am also profoundly grateful to my cherished lifelong friends who have remained constant sources of support and joy: Amir Norooz Alipoor, Reza Moradi, Mohadesse Heydarinejad, Afshin Cheraghi, Mohammad Hassanpour, Reza Safari, Amirhossein Mostofinejad, Amirreza Peymani, Arshia Farzamfar, Jakub Szerszeń, and Geoff Prescott.

Most importantly, I want to express my deepest gratitude to my parents and my brothers, who have been my unwavering pillars of strength throughout this endeavor. Your boundless love, steadfast support, and endless belief in me have shaped who I am today, and without you, this achievement would not have been possible.

Abstract

This thesis examines the strategic and market implications of open market repurchase (OMR) programs through three interconnected studies that advance our understanding of corporate payout policy and market dynamics.

The first chapter provides causal evidence that firms significantly adjust repurchase activity in response to undervaluation. Using price pressure induced by mutual fund flows and leveraging the 2003 mutual fund trading scandal as a natural experiment, I establish that flow-induced valuation shocks causally drive repurchase decisions. Instrumental variable estimates reveal substantially stronger effects than standard regressions, suggesting that the non-fundamental component of fund flows is potent but rare, with its true impact typically masked in conventional analyses. Analysis of long-run stock performance reveals that the well-documented buyback anomaly is primarily driven by repurchase announcements following periods of negative fund flows, highlighting how flow-driven price distortions meaningfully impact corporate payout policy and create predictable patterns in subsequent stock performance.

The second chapter examines the puzzling heterogeneity in completion rates of open market repurchase programs, where some announcing firms execute zero repurchases while others complete their programs rapidly. I propose that managers strategically balance duration-dependent costs of undervaluation against immediate costs of share repurchases, with completion decisions signaling expected timelines of information asymmetry resolution. Using hand-collected SEC filing data spanning 2004-2019, I find that low-completion firms significantly outperform analyst forecasts in years one and two post-announcement, while high-completion firms excel in years three and four, with corresponding patterns in market reactions and long-run returns.

The third chapter examines how OMR programs affect firms' exposure to systematic liquidity shocks. I find that repurchasing firms act as buyers of last resort, experiencing significant but temporary declines in liquidity commonality during programs. This reduction in liquidity commonality is accompanied by decreased liquidity risk, highlighting firms' role in stabilizing against institutional demand and market maker supply variations.

Contents

List of Figures

List of Tables

Chapter 1

Mispricing, Mutual Fund Flows, and Corporate Buybacks

1.1 Introduction

This paper provides causal evidence that firms significantly adjust their share repurchase activity in response to temporary stock undervaluation. Using price pressure from mutual fund flows as a proxy for mispricing, I establish a causal link between non-fundamental valuation shocks and corporate buyback activity. I leverage the 2003 U.S. mutual fund trading scandal as a natural experiment and find that firms exogenously exposed to fund outflows significantly increased both the likelihood and the amount of their share repurchases. Crucially, the causal effect quantified by instrumental variable analysis is substantially larger than standard regressions suggest. This points to a key insight into the nature of fund flows: the non-fundamental component is potent but rare, and its true impact is masked in conventional analyses.

These findings contribute to a long-standing debate in corporate finance regarding the motives behind share repurchases. Announcements of open market share repurchase programs typically elicit favorable reactions from the market, with numerous studies consistently reporting positive cumulative abnormal returns (CARs) in short windows surrounding the announcement (Comment and Jarrell, 1991; Kahle, 2002; Grullon and Michaely, 2004; Leng and Noronha, 2013; Bhattacharya and Jacobsen, 2016). However, the underlying reason for this positive market reaction remains subject to ongoing debate.

Two primary hypotheses have emerged to explain the informational content of repurchase announcements. The first is the *undervaluation hypothesis*, which posits that managers announce share repurchases when they believe their firm's stock is undervalued. Because managers are presumed to have superior information about their firm's intrinsic value, the repurchase announcement serves as a signal to the market that the firm is undervalued.

The second is the *free cash flow hypothesis*, which argues that repurchases serve as a mechanism to mitigate agency problems. Specifically, when firms generate more cash flow than they can invest in positive net present value (NPV) projects, repurchases reduce the likelihood of inefficient capital allocation. In this view, announcement of repurchase program signals firm's commitment to returning excess cash to shareholders rather than squandering it on value-destroying investments.

Empirical work has attempted to adjudicate between these two competing theories, yet findings remain mixed. Grullon and Michaely (2004) directly contrast the two hypotheses by examining both market reactions and subsequent firm performance. They find no evidence of post-announcement improvement in operating performance, but they observe stronger abnormal returns for firms with lower market-to-book ratios, which they interpret as support for the free cash flow hypothesis. Similarly, Wang et al. (2009), in a study of UK firms, report that announcements of actual repurchases also elicit positive market reactions, particularly for firms with low Tobin's Q, but find no relation between the size of the repurchase and the magnitude of the reaction. They argue this evidence is more consistent with the agency-based explanation and inconsistent with the signaling of undervaluation.

On the other hand, Lie (2005) finds improvements in operating performance following repurchase announcements, particularly among firms that follow through with actual repurchases. This suggests that at least some announcing firms deliver stronger-than-expected results, potentially supporting the undervaluation view. Moreover, several studies have documented long-run stock price overperformance for repurchasing firms. Ikenberry et al. (1995) report average abnormal buy-and-hold returns of 12.1% over four years following repurchase announcements, with returns reaching 45.3% for high book-to-market firms. Peyer and Vermaelen (2009) confirm that such long-run overperformance has persisted in more recent periods. Survey evidence further supports the undervaluation hypothesis: in Brav et al. (2005), 86% of CFOs cited undervaluation as one of their primary motivations for initiating a repurchase program.

Despite these empirical efforts, the literature has not reached a consensus on the relative explanatory power of the undervaluation and free cash flow hypotheses. Part of the difficulty stems from the use of noisy and conceptually ambiguous proxies. For instance, Tobin's Q is commonly used as a proxy for investment opportunities, yet it is also employed as a measure of mispricing. The fact that firms with low Q exhibit stronger market reactions could be interpreted as evidence for either hypothesis. Similarly, studies assessing operating performance post-announcement have produced conflicting results, and more fundamentally, often evaluate performance relative to past trends or industry peers rather than against market expectations. If a firm is undervalued precisely because the market holds excessively pessimistic expectations, as long as such firm delivers better-than-expected performance with regard to the expectations at the time of the announcement, it is experiencing an improvement in operating performance from the perspective of the undervaluation hypothesis—even if it underperforms relative to its own past or to its peers.

The identification of suitable counterfactuals is another challenge. Studies often compare repurchasing firms to non-repurchasing firms with similar observable characteristics. However, under the undervaluation hypothesis, repurchasing firms are unique precisely because they are mispriced. This undermines the premise that matched firms are valid controls, as the very absence of a repurchase announcement may indicate that the control firm is not similarly undervalued. Finally, some interpretations in the literature are methodologically problematic. For example, Wang et al. (2009) argue that if repurchases signal managerial confidence, then larger repurchases should elicit stronger reactions. However, the alleviation of agency problems is not binary either, and one could similarly argue that larger repurchases should lead to a greater reduction in agency concerns. Therefore, the lack of a correlation between repurchase size and market reaction could just as well be viewed as inconsistent with the free cash flow hypothesis.

These methodological challenges help explain why the literature has not reached a consensus on the extent to which undervaluation affects corporate buyback activity. Part of the difficulty stems from the fundamental challenge of establishing causality between mispricing and corporate behavior. Establishing a causal link between mispricing and repurchase activity requires identifying shocks that affect prices without simultaneously altering firm fundamentals. To this end, mutual fund flows offer a potentially valuable source of variation to investigate how firms respond to undervaluation, provided that such flows contain at least a partially exogenous component unrelated to firm fundamentals.

A growing body of evidence shows that mutual fund flows can exert significant and persistent influence on asset prices. Coval and Stafford (2007) document that mutual fund outflows can induce price pressure on the shares of firms held in their portfolios, with effects that persist for multiple quarters. Related studies find similar phenomena in other asset markets, including bonds and convertibles (Ellul et al., 2011; Mitchell et al., 2007). At a broader level, mutual fund flows have been shown to distort market efficiency: Lou (2012) links mutual fund flow patterns to momentum effects, while Anton and Polk (2014) demonstrate that common ownership by mutual funds induces excess co-movement in stock prices. Using this insight, I first document a robust negative relationship between mutual fund flow pressure and firm repurchase activity. Firms experiencing net outflows from their mutual fund shareholders in the prior quarter are significantly more likely to announce Open Market Repurchase (OMR) programs and to repurchase larger amounts in the subsequent quarter, even after controlling for firm characteristics known to influence payout decisions.

However, this correlational evidence faces a significant endogeneity concern. Many mutual fund portfolios are highly specialized, often focusing on specific investment styles—such as small-cap versus large-cap, growth versus value—or particular industries. This specialization means that flows into these funds can be driven by changes in the fundamentals of their narrow investment universe, rather than by non-fundamental shocks. This concern is particularly severe in the context of share repurchases because, under a fundamentals-driven story, corporate share buybacks and mutual fund redemptions are two sides of the same coin: both are manifestations of the optimal reallocation of capital. Consider a sector with diminishing growth opportunities. Firms in this sector will generate more free cash flow, leading them to increase share repurchases. Simultaneously, rational investors will direct capital away from this sector, causing outflows from specialized mutual funds. The resulting correlation between outflows and buybacks would thus be entirely mechanical and non-causal.

Indeed, prior work establishing this correlational link faces this challenge. Dudley and Manakyan (2011), for instance, find that firms are more likely to repurchase shares following selling pressure from funds with large outflows, and Chiu and Kini (2014) document a similar pattern at the aggregate level. While this work provides important foundational evidence, the possibility of a spurious relationship driven by underlying fundamentals makes it impossible to draw causal conclusions.

Alternatively, even if a causal link does exist, standard regressions may drastically underestimate its true magnitude. This is because the non-fundamental component of fund flows that drives mispricing may be potent but rare. If most of the variation in fund flows is driven by fundamentals that are unrelated to repurchase decisions, it acts as noise in a regression. This noise attenuates the coefficient on the total flow measure, masking the powerful effect of the small, truly exogenous component. Therefore, a research design capable of not only establishing causality but also isolating this exogenous component is necessary to understand the true relationship between mispricing and corporate buybacks.

To address this endogeneity, I exploit the 2003 U.S. mutual fund trading scandal, which triggered sustained outflows from implicated fund families due to reputational damage rather than changes in the fundamentals of firms they held. This setting provides a natural experiment for identifying the causal effect of flow-induced mispricing on repurchase decisions. Using a difference-in-differences framework, I show that firms with greater pre-scandal exposure to affected fund families exhibit a sharp and statistically significant increase in both OMR announcement rates and repurchase amounts following the scandal, relative to less-exposed firms. Instrumental variables analysis confirms that the flow effect is causal: the exogenous component of flow pressure, driven by scandal exposure, strongly predicts subsequent repurchase activity.

Remarkably, the IV estimates are substantially larger than their OLS counterparts, revealing the severity of the endogeneity bias. This finding suggests that the exogenous component of mutual fund flows is rare but potent—while most flow variation is fundamentally driven, the non-fundamental component exerts substantial causal effects on corporate behavior through mispricing channels. This insight has two important implications. First, it demonstrates that managers respond much more strongly to undervaluation than OLS estimates suggest, providing strong support for the undervaluation hypothesis. Second, it suggests that many papers using mutual fund flows as exogenous variation may significantly underestimate treatment effects due to unaccounted endogeneity.

This logic also echoes insights from Khan et al. (2012), who study the opposite side of the same mechanism. Their work shows that mutual fund inflows can generate non-fundamental buying pressure, creating temporary overvaluation that firms subsequently exploit through equity issuance. Although their setting focuses on issuance rather than repurchases, the underlying idea is closely related: flow-driven price distortions can meaningfully influence corporate financial policy. Importantly, their empirical strategy relies on variation in fund-level inflows rather than a plausibly exogenous shock to flows, and therefore still embeds some fundamentals-driven noise. By providing a causal design that isolates the truly exogenous component of flow pressure, my paper complements this line of work by showing that firms respond far more strongly to flow-induced mispricing than standard regressions suggest.

Building on the finding that mutual fund flows causally affect repurchase activity—and recognizing that flows also constrain the arbitrage capacity of institutional investors—I further examine whether repurchase announcements are interpreted differently by the market depending on the flow environment in which they occur. Specifically, I separate OMR announcements into two groups: those made following quarters of negative mutual fund flow, and those following positive flow. I then analyze the long-run stock price performance of each group over the 48 months following the announcement.

The results reveal a striking divergence. While both groups exhibit modest abnormal returns initially, the long-run outperformance is concentrated entirely among firms announcing buybacks after experiencing fund outflows. These firms display significantly higher cumulative abnormal returns over three- and four-year horizons. This finding suggests that the well-documented "buyback anomaly"—persistent abnormal returns following repurchase announcements (Ikenberry et al., 1995; Peyer and Vermaelen, 2009)—is concentrated among announcements preceded by negative fund flows.

These findings provide a valuable insight into a longstanding puzzle. Peyer and Vermaelen (2009) show that a simple buy-and-hold strategy focusing on repurchasing firms delivers significant abnormal returns over a four-year horizon, a result that remains robust across time periods. They argue that the persistence of this anomaly, despite its simplicity and visibility, is difficult to reconcile with efficient markets. The findings in this paper suggest that mutual fund flow dynamics may impose a key limit to arbitrage. Flows are highly persistent—funds experiencing outflows are likely to experience poor flows and remain cash-constrained for several quarters. Consequently, these investors may be less able to act on undervaluation signals, such as repurchase announcements, thereby allowing mispricing to persist.

This mechanism aligns with the theoretical perspective of Pulvino (1998), who emphasizes that the price at which transactions occur depends critically on the presence and capacity of willing buyers. When institutional investors are constrained by redemption pressures, their ability to exploit mispricing—even when signaled by managerial actions—may be limited. Thus, mutual fund outflows may not only be associated with greater undervaluation but also contribute to a slower correction of that undervaluation.

Further evidence supporting this view comes from an analysis of short-run announcement effects. In Table 1.11, I compare abnormal returns, trading volume, and turnover around the announcement day for each subgroup. Interestingly, while firms announcing after outflows exhibit stronger long-run performance, their announcements are met with significantly weaker immediate price and trading reactions. This muted response suggests that the market, particularly the constrained mutual fund segment, may be slower to process and act on the undervaluation signal in the presence of flow-induced frictions.

This paper also contributes to the broader literature examining the real effects of financial markets on corporate behavior. A growing number of studies have explored how market frictions—particularly those stemming from institutional investor behavior—can influence firm decisions and outcomes. For instance, Edmans et al. (2012) exploit mutual fund outflows as an exogenous shock to valuation to study their impact on takeover activity. Similarly Hau and Lai (2013) examine the 2007–2009 financial crisis, showing that forced stock selling by distressed funds led to underpricing that caused affected firms to cut back on both investment and employment. In this spirit, my findings demonstrate that variation in mutual fund flows exerts a significant influence on firms' repurchase behavior, highlighting how capital market dynamics can shape real corporate actions even in the absence of fundamental shocks.

1.2 Data

1.2.1 Mutual Fund Data

I source quarterly mutual fund holdings from the Thomson Reuters CDA/Spectrum database for the 1994–2020 period. This database aggregates data from both required SEC filings and voluntary fund disclosures. A common issue with this data is that a fund's reported holdings may be valid for a date that precedes the end-of-quarter filing date. To address this timing mismatch, I adopt the standard assumption in the literature and assume no trading occurs between the report date and the quarter-end, adjusting for any stock splits.

To augment the holdings data, I source fund-level characteristics from the CRSP Survivorship-Bias-Free U.S. Mutual Fund Database, including total net assets (TNA), net returns, and expense ratios. I then merge the CRSP and CDA/Spectrum datasets using the standard MFLinks file. For funds with multiple share classes, I aggregate the TNA across all classes to arrive at a single fund-level value and compute a TNA-weighted average return. As a final data quality check, I apply two screens: I require a fund's TNA to be above $1 million, and I exclude observations where the TNA reported in the two datasets differs by more than a factor of two.

To measure the flow-induced pressure on a given stock, I first calculate the net capital flow for each fund in my sample. I use the standard formula, as established in prior literature (Chevalier and Ellison, 1997; Sirri and Tufano, 1998; Lou, 2012):

$$flow_{i,t} = \frac{TNA_{i,t} - TNA_{i,t-1}(1 + RET_{i,t}) - MGN_{i,t}}{TNA_{i,t-1}}, \tag{1.1}$$

In this calculation, the flow for fund i in quarter t is the change in its total net assets (TNA) after accounting for capital appreciation ($RET_{i,t}$) and assets acquired through mergers ($MGN_{i,t}$). For fund initiations or liquidations, flows are set equal to the fund's initial or final TNA, respectively. To handle mergers where the exact date is not reported, I use the target fund's last NAV report date to approximate the timing of the asset transfer and assume all flows occur at quarter-end.

To measure fund flow pressure at the firm level, I construct the Flow-Induced Trading (FIT) variable for firm j in quarter t following the methodology of Lou (2012):

$$FIT_{j,t} = \frac{\sum_i shares_{i,j,t-1} \cdot flow_{i,t}}{\sum_i shares_{i,j,t-1}}, \tag{1.2}$$

where $shares_{i,j,t-1}$ denotes the number of shares of firm j held by fund i at the end of quarter $t-1$. This variable captures the average flow-weighted pressure exerted by mutual funds holding the firm's stock.

1.2.2 Other Data

Data on open market repurchase (OMR) program announcements are sourced from the Securities Data Corporation (SDC) Mergers, Acquisitions, and Repurchases database. I retain announcements for securities listed on the NYSE, NASDAQ, or AMEX, provided that corresponding return, accounting, and mutual fund holdings data are available from CRSP, COMPUSTAT, and CDA/Spectrum. These criteria yield a sample of 20,211 OMR announcements between 1994 and 2020.

To measure actual repurchase activity, I construct a firm-quarter level variable, $QtrRepAmt_{j,t}$, defined as:

$$QtrRepAmt_{j,t} = \max(0, -net\_issue_{j,t}), \tag{1.3}$$

where $net\_issue_{j,t} = (\log(adj\_SHROUT_{j,t}) - \log(adj\_SHROUT_{j,t-1})) \times 100$ is the percentage quarterly log change in split-adjusted shares outstanding. The variable adj_SHROUT is obtained from monthly CRSP; for each quarter, I use the final monthly observation to compute the quarter-to-quarter change. $QtrRepAmt$ captures repurchases by taking the negative of net issuance when firms reduce their outstanding shares and sets it to zero otherwise. This measure ensures that only quarters with net repurchase activity (i.e., a decline in shares outstanding) are counted as positive values, consistent with the goal of isolating actual buyback execution.

I obtain firm-level accounting data from COMPUSTAT and stock return data from CRSP, and merge these with the mutual fund holdings data to construct the control variables used in the analysis. All firm-level variables are measured at the end of quarter t. The variable $Own_{j,t}$ is the fraction of firm j's shares outstanding held by all mutual funds at quarter-end. $BM_{j,t}$ is the book-to-market ratio, defined as the book value of equity divided by the market value of equity. $Debt_{j,t}$ is total debt (short-term plus long-term) scaled by total assets, while $Cash_{j,t}$ is operating cash flow divided by total debt. $Size_{j,t}$ denotes the firm's market capitalization decile (1 = smallest, 10 = largest), computed each quarter across firms in my dataset. Finally, $ExRet_{j,t}$ is the firm's raw stock return in quarter t minus the CRSP value-weighted market return over the same period.

Table 1.1 provides summary statistics for the key variables used in the analysis. For my main independent variable, Flow-Induced Trading (FIT), Table 1.2 presents a more detailed, year-by-year look at its cross-sectional distribution.

1.3 Empirical Findings

This section presents the core empirical analysis examining the relationship between mutual fund flows and corporate share repurchase activity. I begin by establishing baseline correlations using the Fama-MacBeth methodology (Fama and MacBeth, 1973), before turning to a natural experiment in the next section to investigate causality.

1.3.1 Fund Flows and Corporate Repurchase Activity: Baseline Analysis

I begin by analyzing whether mutual fund flows in the previous quarter influence a firm's repurchase behavior in the current quarter. Specifically, I examine two dimensions of repurchase activity. The first is the likelihood of announcing an Open Market Repurchase

(OMR) program, captured by a binary indicator $OMR_{j,t}$ that equals 1 if firm j announces an OMR program in quarter t, and 0 otherwise. The second is the magnitude of actual share repurchases, measured by $QtrRepAmt_{j,t}$, defined as the maximum of zero and the negative of the percentage quarterly log change in split-adjusted shares outstanding ($\max(0, -NetIssue_{j,t})$).

My key independent variable measures the trading pressure exerted on firm j due to mutual fund flows in the preceding quarter ($t-1$). Following the literature, I construct a measure of Flow-Induced Trading ($FIT_{j,t-1}$), which aggregates the individual flows of mutual funds weighted by their holdings in firm j as of the end of quarter $t-2$. From this, I derive two variables: (i) a binary indicator $FlowSign_{j,t-1}$, equal to 1 if $FIT_{j,t-1} > 0$ and 0 otherwise, capturing the direction of flow pressure; and (ii) a continuous variable $FlowLevel_{j,t-1}$, which normalizes $FIT_{j,t-1}$ cross-sectionally each quarter to have mean zero and unit variance, capturing the magnitude and direction of pressure relative to the market.

I control for a standard set of firm characteristics known to influence payout and financing decisions, measured at the end of quarter $t-1$. These include mutual fund ownership ($Own_{j,t-1}$), the book-to-market ratio ($BM_{j,t-1}$), the ratio of cash flow to total debt ($Cash_{j,t-1}$), the debt-to-assets ratio ($Debt_{j,t-1}$), firm size decile ($Size_{j,t-1}$), and prior-quarter excess stock return ($ExRet_{j,t-1}$). Except for $Size_{j,t-1}$ (a decile rank from 1 to 10) and $ExRet_{j,t-1}$ (already adjusted for market returns), all continuous independent variables are normalized cross-sectionally each quarter to have mean zero and standard deviation one.

I include these controls because they have been shown to influence corporate payout policy and share repurchases. I incorporate $Own_{j,t-1}$ as prior research finds that institutional investors affect payout choices, with higher ownership associated with greater use of both dividends and repurchases (Grinstein and Michaely, 2005; Crane et al., 2016). The book-to-market ratio $BM_{j,t-1}$ proxies for growth opportunities and free cash flow, and has also been used as a measure of relative valuation in prior studies. $Debt_{j,t-1}$ and $Cash_{j,t-1}$ capture financial flexibility and constraints, which can shape a firm's ability and propensity to repurchase shares. Finally, I include $ExRet_{j,t-1}$ because prior work documents that firms are more likely to initiate repurchases following periods of poor stock performance (Stephens and Weisbach, 1998; Peyer and Vermaelen, 2009).

To estimate the average relationship between fund flows and repurchase activity over the sample period (1994–2020), I implement the two-step Fama-MacBeth procedure (Fama and MacBeth, 1973). In the first step, I run quarterly cross-sectional regressions. For the binary outcome $OMR_{j,t}$, I estimate a Probit model; for the continuous outcome $QtrRepAmt_{j,t}$, I estimate an OLS model. The general specification is:

$$Outcome_{j,t} = \alpha_t + \beta_t FlowMeasure_{j,t-1} + \boldsymbol{\gamma_t}' Controls_{j,t-1} + \epsilon_{j,t} \tag{1.4}$$

where $Outcome_{j,t}$ is either $OMR_{j,t}$ or $QtrRepAmt_{j,t}$, and $FlowMeasure_{j,t-1}$ denotes either $FlowSign_{j,t-1}$ or $FlowLevel_{j,t-1}$. Coefficients α_t, β_t, and $\boldsymbol{\gamma}_t$ are estimated separately each quarter.

In the second step, I compute time-series averages of the estimated coefficients. For the OLS regressions, I report the mean of the quarterly coefficients. For the Probit regressions, I first compute the Average Marginal Effect (AME) for each variable in each quarter, then report the time-series average of these AMEs. I assess statistical significance using Newey and West (1987) standard errors with four lags, accounting for serial correlation in the quarterly estimates.

Table 1.3 presents the Fama-MacBeth Probit results for the likelihood of announcing an OMR. Across all specifications (Columns 1–6), both $FlowSign$ and $FlowLevel$ are strongly statistically significant. In Column (3), which includes the full set of controls, the AME on $FlowSign$ implies that firms experiencing negative mutual fund flows in the prior quarter ($FlowSign = 0$) are 0.52 percentage points more likely to announce an OMR than those experiencing positive flows. Given the unconditional probability of an OMR announcement in the sample is approximately 4.5%, this effect represents an increase of over 11% relative to the baseline, suggesting a meaningful economic impact. Similarly, the AME on $FlowLevel$ in Column (6) implies that a one standard deviation decrease in flow pressure increases the likelihood of an OMR announcement by 0.37 percentage points.

Table 1.4 presents the Fama-MacBeth OLS results for the amount of shares repurchased. Consistent with the OMR results, firms experiencing weaker or negative flow pressure repurchase significantly more in the following quarter. In Column (3), the coefficient on $FlowSign$ is -0.116 (t-stat = -4.44), implying that negative flow pressure is associated with a 0.116 percentage point increase in quarterly repurchase amount. Given the average quarterly repurchase amount is 0.58% (with a standard deviation of 1.18%), this represents roughly 20% of the mean and 10% of the standard deviation—again indicating an economically substantial effect. In Column (6), a one standard deviation decline in $FlowLevel$ is associated with a 0.096 percentage point increase in repurchase amount (t-stat = -5.23).

The control variables display expected patterns. Higher cash flow ($Cash$) is strongly associated with both a higher probability of announcing an OMR and larger repurchase amounts. Larger firms ($Size$) are more likely to repurchase shares, while more highly leveraged firms ($Debt$) are less likely to do so—consistent with financial constraint explanations. Firms with lower prior-quarter returns ($ExRet$) are more likely to announce OMRs and repurchase more. The book-to-market ratio (BM) is positively related to repurchase amounts, and marginally associated with OMR announcements. Mutual fund ownership (Own) is positively related to repurchase activity in simple specifications, but its effect becomes statistically insignificant once the full set of controls is included (Columns 3 and 6), suggesting that other firm characteristics absorb much of its explanatory power.

Together, the Fama-MacBeth results in Tables 1.3 and 1.4 provide strong preliminary evidence of a negative relationship between prior-quarter mutual fund flow pressure and both the likelihood and magnitude of corporate repurchases in the following quarter, even after controlling for a comprehensive set of firm characteristics. In the next section, I address endogeneity concerns and assess the causal effect of fund flow pressure using a natural experiment.

1.3.2 Causal Inference: The 2003 Mutual Fund Scandal

While the preceding Fama-MacBeth analysis documents robust correlations between mutual fund flows and corporate share repurchase activity, interpreting these relationships as causal is complicated by endogeneity concerns. In particular, mutual fund flows may not be orthogonal to the underlying fundamentals or growth prospects of the firms held in their portfolios. This concern is amplified by the fact that mutual funds often specialize along well-defined dimensions—such as investment style (e.g., value versus growth), firm size (e.g., small-cap versus large-cap), or industry focus (e.g., technology, healthcare, or semiconductors). These specializations can cause flow shocks to be systematically related to changing firm fundamentals.

To address this issue, I exploit a natural experiment stemming from the 2003 U.S. mutual fund scandal. In September 2003, several major fund families were implicated in illegal trading practices, including market timing and late trading. Over the following month, 25 fund families settled regulatory charges. The scandal triggered large, sustained investor withdrawals from the implicated fund families—outflows driven by reputational concerns, rather than the fundamentals of their portfolio firms. As documented by Kisin (2011), these funds experienced outflows averaging 14.1% in the first year and 24.3% within two years, with redemptions continuing through 2006. In contrast, non-implicated funds saw continued asset growth. These scandal-driven outflows provide plausibly exogenous variation in mutual fund flow pressure.

To measure firm-level exposure to this shock, I construct a scandal exposure variable based on mutual fund holdings at the end of Q3 2003. For each firm j, similar to Anton and Polk (2014), I calculate $Exposure_j$ as the fraction of its mutual fund–owned shares held by scandal-implicated fund families. Specifically, this is defined as the number of shares in firm j held by scandal-implicated funds divided by the total number of shares in firm j held by all mutual funds at that time. I then define a binary indicator, $HighlyExposed_j$, equal to 1 if $Exposure_j$ is above the cross-sectional median, and 0 otherwise. This classification is fixed based on ownership at the time of the scandal and allows me to compare firms differentially affected by scandal-induced outflows.

I begin by visually assessing whether firms with high and low scandal exposure exhibited similar repurchase behavior prior to the shock. Figures 1.1 and 1.2 plot the average OMR announcement rates and average quarterly repurchase amounts, respectively, for the two groups. In the pre-scandal period (before the dashed vertical line), the two groups not only exhibit parallel trends but also display nearly identical levels—suggesting a strong form of pre-treatment comparability. Following the scandal, both groups show an increase in repurchase activity, but the increase is substantially more pronounced for firms with high exposure to the scandal-implicated funds. This results in a clear divergence in both OMR rates and repurchase amounts between the two groups. Over time, the gap gradually narrows, consistent with the temporary nature of the scandal-induced shock. These patterns provide strong visual support for the parallel trends assumption and the validity of the scandal as a quasi-experimental shock.

I next formalize this analysis using a Difference-in-Differences (DiD) approach. My estimating equation is:

$$\begin{aligned} Outcome_{j,t} = \beta_0 + \beta_1 HighlyExposed_j + \beta_2 PostScandal_t \\ + \beta_3 (HighlyExposed_j \times PostScandal_t) + \delta' Controls_{j,t-1} + \nu_{j,t} \end{aligned} \tag{1.5}$$

The outcome variable, $Outcome_{j,t}$, is either the binary $OMR_{j,t}$ indicator or the continuous $QtrRepAmt_{j,t}$. The indicator $PostScandal_t$ equals 1 in post-scandal quarters (2003q4 onward) and 0 otherwise. The interaction term captures the differential change in outcomes for highly exposed firms after the scandal. Control variables are identical to those used in the baseline analysis, and I include quarter fixed effects and industry fixed effects (based on Fama–French 12 classifications) in some specifications. Standard errors are clustered at the firm level.

I estimate this model using quarterly data from 2000q1 to 2006q4, providing a sufficient pre- and post-scandal window. When the outcome is $QtrRepAmt_{j,t}$, I use OLS. When the outcome is $OMR_{j,t}$, I use a Probit model and report the average marginal effect for the interaction term. Tables 1.5 and 1.6 present results for OMR announcements and repurchase amounts, respectively. Each table includes four specifications: (1) core DiD terms only, (2) adds mutual fund ownership (Own), (3) includes the full control set, and (4) adds quarter and industry fixed effects.

In Table 1.5, the AME of the interaction term in the full specification is 0.01698 (z = 5.35), indicating that highly exposed firms were approximately 1.7 percentage points more likely to announce an OMR after the scandal relative to the low-exposure group. Given a baseline OMR rate of approximately 4.5%, this effect is economically meaningful. The estimate is stable across all specifications. In Table 1.6, the full-specification coefficient is 0.1017 (t =6.35), implying that the average quarterly repurchase amount increased by over 0.10 percentage point for highly exposed firms. Again, the result is highly statistically and economically significant, and consistent across specifications.

Together, these findings provide compelling causal evidence that the scandal-induced outflows had a substantial effect on firms' repurchase behavior, both in terms of the likelihood of announcing buybacks and the scale of repurchases undertaken. The stability of these estimates across all specifications in both tables further suggests that the results are robust to the inclusion of a comprehensive set of firm characteristics as well as time and industry fixed effects.

While DiD isolates the effect of scandal exposure and is useful in establishing the causal link, it does not directly quantify the impact of fund flows. To quantify the causal effect of flows, I employ an instrumental variable (IV) approach, using $HighlyExposed_j$ as an instrument for $FlowSign_{j,t-1}$. I estimate 2SLS regressions for $QtrRepAmt_{j,t}$ and IV Probit models for $OMR_{j,t}$, focusing on the post-scandal window (2003q4–2006q4). This window allows me to leverage the exogenous variation induced by the scandal while acknowledging the likely temporary nature of the shock. Controls and fixed effects match those used in the DiD analysis, and standard errors are again clustered at the firm level.

Table 1.7 presents the first-stage results. Across all specifications, the coefficient on the $HighlyExposed_j$ indicator is negative and highly statistically significant, indicating that firms with greater exposure to scandal-implicated funds were significantly more likely to experience negative mutual fund flow in the quarters following the scandal. This confirms that $HighlyExposed_j$ is a strong and relevant instrument for $FlowSign_{j,t-1}$. The F-statistics for the excluded instrument exceed the conventional threshold of 10 in all specifications, ranging from 26.94 to 104.45, thereby alleviating concerns about weak instrument bias and reinforcing the validity of my identification strategy.

Tables 1.8 and 1.9 present the second-stage estimates from the IV regressions, alongside their non-IV counterparts. In both cases, the IV coefficients on $FlowSign$ are negative and statistically significant, indicating that negative fund flow pressure leads to an increase in repurchase activity. More importantly, the IV estimates are substantially larger in magnitude than the corresponding Probit and OLS coefficients, suggesting that the baseline models understate the true causal effect.

In Table 1.8, which reports Average Marginal Effects (AMEs) for the binary OMR outcome, the IV Probit estimate in Column 7 is -0.0338 (z = -4.06). This implies that experiencing negative flow—instrumented by exogenous variation from scandal exposure—increases the likelihood of announcing an OMR program by 3.38 percentage points. This effect is over three times larger than the corresponding AME from the standard Probit model in Column 8, which is -0.0091. Relative to the sample's unconditional OMR rate of approximately 4.5%, the IV estimate reflects a highly economically meaningful response.

A similar, even more striking pattern emerges in Table 1.9, which presents results for the continuous repurchase amount. The IV coefficient in Column 7 is -0.3445, compared to just -0.0501 in the corresponding OLS specification in Column 8. This large discrepancy provides a key insight into the nature of fund flows. It suggests that while most flow variation is likely driven by fundamentals, the non-fundamental component is potent but rare. OLS estimates, which are based on the noisy total flow measure, suffer from severe attenuation bias because firms do not respond to fundamentals-driven price changes, only to mispricing. The IV strategy, by isolating the pure mispricing shock from the scandal, reveals the true, powerful magnitude of the corporate response.

Taken together, these IV results provide strong causal evidence that firms respond much more decisively to undervaluation than standard regressions suggest. The magnitude of this causal effect underscores how central perceived mispricing is to the corporate decision-making process for share repurchases. Furthermore, it offers a broader methodological insight: studies across finance that use total fund flows as a proxy for non-fundamental shocks may be significantly underestimating the true economic impact of mispricing. The true effect only becomes apparent once the potent, exogenous component of flows is isolated.

1.3.3 Long-Run Performance Following OMR Announcements

The preceding analyses provide causal evidence that mutual fund outflows increase the likelihood and scale of firms' open market repurchase (OMR) activity. These findings raise a natural follow-up question: do firms that announce repurchase programs following negative fund flows exhibit stronger undervaluation, and if so, does this manifest in superior long-run stock performance?

There are two key reasons to expect such a pattern. First, if negative flows contribute to greater mispricing, then repurchases following outflows may be targeting more undervalued shares. Second, because mutual fund flows are highly persistent (Lou, 2012), funds experiencing outflows are likely to remain cash-constrained in subsequent quarters. This limits their capacity to respond to repurchase signals in the near term, which in turn may delay price correction and extend the return window. Thus, firms announcing OMRs after experiencing negative flows might display stronger long-run abnormal returns as prices gradually revert to fundamentals.

To examine this, I divide my sample of OMR announcements into two groups based on the sign of the prior-quarter fund flow: those preceded by negative ($FlowSign = 0$) and those preceded by positive flow ($FlowSign = 1$). I then track long-run abnormal returns over a 48-month window following the announcement month.

Following Peyer and Vermaelen (2009), I use the Fama and French (1993) three-factor model along with Ibbotson's Returns Across Time and Securities (RATS) methodology to estimate abnormal returns. For each month τ in event time ($\tau = 1, \ldots, 48$), I run a cross-sectional regression across all firms that reach that event month:

$$R_{j,t} - R_{f,t} = a_\tau + b_\tau(R_{m,t} - R_{f,t}) + c_\tau SMB_t + d_\tau HML_t + \epsilon_{j,t} \tag{1.6}$$

Here, $R_{j,t}$ is the return on stock j in calendar month t, and $R_{f,t}$ is the risk-free rate. The estimated intercept a_τ represents the average abnormal return in event month τ. I calculate Cumulative Abnormal Returns (CARs) over various windows by summing these monthly alphas: $CAR[1, 12]$, $CAR[1, 24]$, $CAR[1, 36]$, and $CAR[1, 48]$.

Table 1.10, Panel A, reports CARs separately for the $FlowSign = 0$ and $FlowSign = 1$ subgroups. A striking pattern emerges. In the short run (first 12 months), firms announcing after positive flows outperform slightly, with a CAR of 2.87% (t = 2.09) versus 0.84% (t=0.82) for the negative-flow group, possibly reflecting momentum effects from recent inflows (Lou, 2012). However, over longer horizons, the pattern reverses. By 36 months, the negative-flow group shows a highly significant CAR of 10.76% (t = 3.27), compared to an insignificant 5.63% for the positive-flow group. At 48 months, the gap widens further: 14.33% (t = 3.87) versus 6.16% (t = 1.49). These results suggest that the long-run outperformance typically associated with repurchase announcements is concentrated among firms announcing after negative flows.

These findings raise a further question: conditional on experiencing similar prior flows, does the repurchase announcement itself provide additional explanatory power for long-run performance? To investigate this, for each announcing firm I identify a matched non-announcing firm that is similar in terms of its ownership and flow characteristics. Specifically, I require the matched firm to belong to the same mutual fund ownership decile and the same $FlowLevel$ decile, based on characteristics in the quarter prior to the announcement. If multiple firms satisfy these conditions, I further select among them by prioritizing firms in the same industry, then (if necessary) those in the same size decile, and finally the firm with the closest book-to-market ratio. Having identified a matched counterpart for each announcing firm, I compute the *flow-adjusted excess return* as the difference between the announcing firm's return and that of its matched peer in each event month. I then apply the same RATS procedure to estimate monthly abnormal returns, and as in Panel A, compute CARs over the standard windows (e.g., $[1, 12]$, $[1, 24]$, etc.) by summing the monthly alphas.

Panel B of Table 1.10 presents the results. These findings yield several important insights. First, consistent with the raw CARs in Panel A, the long-run overperformance associated with OMR announcements remains concentrated among firms announcing after negative flows, even when benchmarked against firms with similar flow, ownership, and characteristic profiles. Second, these results demonstrate that repurchase announcements retain substantial explanatory power for future overperformance even after conditioning on flows. Relative to matched non-announcing firms that experienced similar prior flows, announcing firms exhibit markedly stronger long-run returns: adjusting for the matched firms' performance reduces the 48-month CAR only modestly, from 14.33% to 10.53%. This pattern is consistent with earlier findings that the exogenous component of flows is potent but rare. If it is primarily the exogenous, price-pressure component of flows that moves prices away from fundamental value—and if firms selectively repurchase when they perceive such mispricing—then announcing firms are more likely to have experienced exogenous flow shocks than their matched non-announcing peers. In that light, it is less surprising that similar observed flows may correspond to different valuation dynamics in subsequent years.

While firms announcing after negative flows eventually deliver stronger long-run returns, the presence of constrained mutual fund owners raises the possibility that the immediate market reaction to their announcements may be more muted relative to firms announcing after positive flows. To investigate this further, I examine market reactions in the days surrounding the OMR announcement. I calculate three metrics over the [-1, +2] event window: (1) $CAR[-1, +2]$, based on daily excess returns; (2) *Abnormal Volume*, defined as the difference between the average of $\log(1 + \text{Volume})$ over the event window and its average over the pre-event window [-252, -1]; and (3) *Abnormal Turnover*, defined as the difference between the average of $\log(\text{Turnover} + c)$ over the event window and the corresponding average over the pre-event window, where Turnover is daily volume divided by shares outstanding, and $c = 0.0001275$ is a small constant added to accommodate zero-turnover days (Bhattacharya and Jacobsen, 2016).

Table 1.11 presents these results. Both flow subgroups show statistically significant positive reactions in price and trading activity. However, announcements following positive flows exhibit significantly stronger immediate effects: the mean CAR[-1,+2] is 1.15 percentage points higher, and abnormal volume and turnover are also significantly larger.

This contrast highlights the dual role of fund flows. Negative flows may be associated with greater undervaluation and more limited arbitrage capacity, setting the stage for stronger long-run performance. Yet positive flows appear to enable a more vigorous short-term reaction, possibly due to better liquidity or fewer constraints on institutional investors. These results underscore that the flow environment not only shapes repurchase decisions but also affects how quickly and strongly the market responds to those decisions.

1.4 Summary and Conclusion

This paper revisits the fundamental question of how corporate share repurchase policy responds to stock misvaluation. While the idea that firms may increase their buyback activity when they are undervalued has been a longstanding notion in the corporate finance literature, empirically testing this relationship has been notoriously difficult. Prior studies have been constrained by a reliance on noisy proxies for mispricing and by the fundamental challenge of distinguishing a firm's response to true undervaluation from its reaction to changes in underlying fundamentals. In this paper, I offer a new empirical strategy that uses price pressure from institutional fund flows, combined with a natural experiment, to causally identify and quantify this crucial relationship.

Using measures of flow-induced trading pressure, I show that firms experiencing negative mutual fund flows in the prior quarter are significantly more likely to announce OMR programs and to repurchase larger amounts of shares. To establish causality, I exploit the 2003 U.S. mutual fund trading scandal as a natural experiment. The resulting IV analysis reveals that the causal effect of flows on repurchase activity is substantially larger than standard regressions suggest, reaffirming concerns about endogeneity in conventional regressions. The magnitude of this response is economically significant: for example, in the fully specified IV model with controls and time fixed effects, firms repurchase 0.358 percentage points more of their shares outstanding in response to an exogenous negative flow shock. This large, causal response confirms that flow-driven mispricing meaningfully shapes corporate payout decisions.

I also document striking differences in long-run stock performance following repurchase announcements, depending on the prior-quarter flow environment. Firms that announced buybacks after experiencing outflows earn significantly higher abnormal returns over the subsequent four years than those announcing after inflows. However, despite this stronger long-run performance, the short-run market reaction and trading intensity around announcement day are significantly weaker for the outflow group. This contrast underscores a key insight: mutual fund flows not only generate valuation pressure but also constrain the ability of these investors to respond to repurchase signals. These findings help explain the persistence of the buyback anomaly and point to limits to arbitrage as a central factor in the slow correction of mispricing.

Taken together, this paper contributes to the literature on corporate finance and asset pricing by offering new evidence on how valuation shocks shape firm behavior, and how investor frictions influence the market's response to corporate signals. It also adds to the broader literature on the real effects of financial markets, showing that flow-driven price distortions can meaningfully impact corporate payout policy and subsequent price dynamics.

1.5 Figures

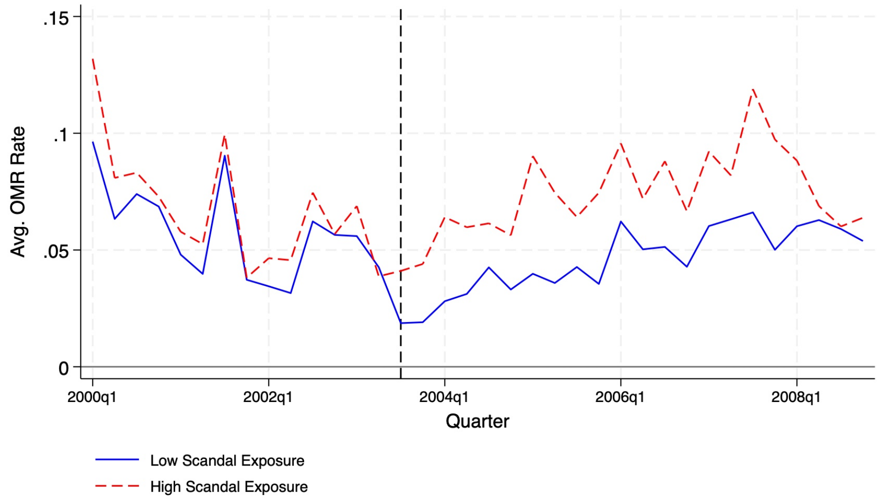

Figure 1.1. OMR Announcement Rate by Scandal Exposure Group

This figure plots the average quarterly open market repurchase (OMR) announcement rate—defined as the fraction of firms announcing an OMR—for firms with high scandal exposure ($HighlyExposed = 1$, dashed red line) and low scandal exposure ($HighlyExposed = 0$, solid blue line). Scandal exposure is based on mutual fund ownership by implicated fund families as of Q3 2003. The vertical dashed line marks the scandal quarter (Q3 2003).

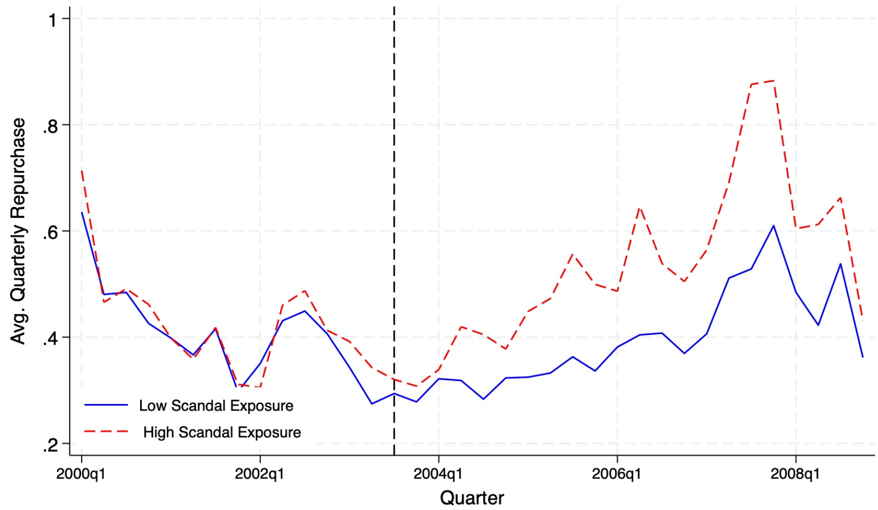

Figure 1.2. Average Quarterly Repurchase by Scandal Exposure Group

This figure plots the average quarterly share repurchase amount for firms with high scandal exposure ($HighlyExposed = 1$, dashed red line) and low scandal exposure ($HighlyExposed = 0$, solid blue line). Scandal exposure is based on mutual fund ownership by implicated fund families as of Q3 2003. The vertical dashed line marks the scandal quarter (Q3 2003).

1.6 Tables

Table 1.1. Summary Statistics for Key Variables

This table presents summary statistics for the key variables used in the regression analyses. The sample consists of firm-quarter observations from 1994 to 2020. $QtrRepAmt$ is the quarterly repurchase amount, defined as the maximum of zero and the negative of the percentage quarterly log change in shares outstanding. FIT is the Flow-Induced Trading for a firm, defined as the weighted average of the capital flows of its mutual fund shareholders, where weights are the fund's lagged holdings of the firm's stock. Control variables are defined as follows: Own is the fraction of a firm's shares outstanding held by mutual funds; BM is the book-to-market ratio; $Cash$ is the ratio of cash flow to total debt; $Debt$ is the ratio of total debt to total assets; $Size$ is the firm's market capitalization decile, ranked from 1 to 10 each quarter; and $ExRet$ is the firm's raw stock return in the prior quarter minus the value-weighted market return.

Variable	**N**	**Mean**	**Std. Dev.**	**P25**	**Median**	**P75**
$QtrRepAmt$ (%)	427,185	0.588	1.183	0.000	0.020	0.664
FIT	427,185	0.013	0.074	-0.016	0.003	0.030
Control Variables						
Own (%)	427,185	10.101	11.431	1.476	5.521	14.826
BM	427,185	0.658	0.601	0.293	0.522	0.837
$Cash$	427,185	0.063	0.608	0.012	0.108	0.257
$Debt$	427,185	0.215	0.210	0.032	0.167	0.335
$Size$ (1-10)	427,185	5.450	2.872	3	5	8
$ExRet$ (%)	427,185	-1.021	12.363	-10.220	-1.210	4.555

Table 1.2. Summary Statistics for Flow-Induced Trading (FIT) by Year

This table presents the cross-sectional distribution of the Flow-Induced Trading (FIT) variable for each year in the sample from 1994 to 2020. The sample consists of firm-quarter observations. For each year, I report the number of observations (N), the mean, median, standard deviation (Std. Dev.), the 25th (P25) and 75th (P75) percentiles, and the fraction of firm-quarters with positive FIT. The final row provides summary statistics for the full sample period. FIT is defined as the weighted average of the flows of a firm's mutual fund shareholders, where weights are the fund's lagged holdings of the firm's stock, as defined in Equation (1.2).

Year	N	Mean	Median	Std. Dev.	P25	P75	Fraction Positive (%)
1994	15,267	0.036	0.022	0.077	-0.002	0.054	73.6
1995	15,587	0.043	0.035	0.074	0.011	0.068	81.0
1996	13,280	0.037	0.023	0.093	-0.008	0.057	68.3
1997	16,907	0.046	0.032	0.100	0.001	0.072	76.0
1998	17,321	0.010	0.000	0.106	-0.035	0.035	50.7
1999	16,314	-0.002	-0.018	0.123	-0.063	0.025	37.8
2000	19,431	0.025	0.012	0.070	-0.012	0.043	62.5
2001	17,906	0.054	0.033	0.100	0.000	0.074	75.2
2002	15,977	0.003	-0.006	0.080	-0.026	0.019	43.9
2003	16,908	0.047	0.033	0.091	0.012	0.059	83.9
2004	17,531	0.022	0.019	0.066	-0.004	0.041	71.3
2005	17,000	0.005	0.002	0.089	-0.025	0.034	51.7
2006	17,777	0.004	0.000	0.054	-0.018	0.018	50.1
2007	17,648	-0.013	-0.011	0.053	-0.033	0.006	32.7
2008	17,185	0.000	-0.006	0.052	-0.024	0.015	41.8
2009	15,832	0.017	0.008	0.060	-0.011	0.036	60.6
2010	14,713	0.009	0.002	0.052	-0.017	0.026	53.2
2011	13,221	-0.012	-0.013	0.051	-0.027	0.002	27.6
2012	13,143	0.003	-0.002	0.046	-0.017	0.018	45.8
2013	13,933	0.015	0.010	0.051	-0.003	0.027	69.6
2014	13,651	-0.002	-0.004	0.050	-0.018	0.011	42.9
2015	13,648	-0.008	-0.009	0.039	-0.021	0.003	30.5
2016	12,999	0.004	0.002	0.036	-0.013	0.018	53.3
2017	14,894	-0.004	-0.005	0.031	-0.013	0.004	35.8
2018	16,339	-0.001	-0.002	0.037	-0.012	0.010	46.2
2019	16,397	-0.008	-0.008	0.037	-0.016	-0.001	23.3
2020	16,376	0.003	-0.006	0.061	-0.021	0.020	43.4
Full Sample	427,185	0.013	0.003	0.074	-0.016	0.030	53.4

Table 1.3. Fama-MacBeth Regressions of OMR Announcement Likelihood on Fund Flows

This table presents Fama and MacBeth (1973) estimates examining the relationship between prior-quarter mutual fund flow pressure and the likelihood of announcing an Open Market Repurchase (OMR) program during the period 1994–2020. The dependent variable, OMR, is a binary indicator equal to 1 if a firm announces an OMR program in a given quarter, and 0 otherwise. I follow a two-step procedure. First, a cross-sectional Probit regression is estimated each quarter. Second, I compute the Average Marginal Effect (AME) of each variable within each quarterly regression. The table reports the time-series average of these AMEs. Columns (1)–(3) use a binary flow pressure measure, $FlowSign$, equal to 1 for positive prior-quarter flow and 0 otherwise. Columns (4)–(6) use the continuous measure $FlowLevel$, which captures the standardized magnitude of prior-quarter flow pressure. Control variables include mutual fund ownership (Own), book-to-market ratio (BM), cash flow to debt ratio ($Cash$), debt to assets ratio ($Debt$), size decile ($Size$), and prior-quarter excess return ($ExRet$). All continuous independent variables –except for $ExRet$ and $Size$– are standardized each quarter to have mean zero and standard deviation one. $Size$ is ranked from 1 to 10; $ExRet$ is the firm's return minus the value-weighted market return. Reported test statistics (t-values) are based on Newey and West (1987) standard errors using 4 lags. Significance levels: * p<0.10, ** p<0.05, *** p<0.01.

	$FlowSign$			$FlowLevel$		
Dependent Variable:	OMR Announcement (OMR)					
	(1)	(2)	(3)	(4)	(5)	(6)
$FlowSign$	-0.00643***	-0.00569***	-0.00521***			
	(-9.67)	(-6.29)	(-5.52)			
$FlowLevel$				-0.00481***	-0.00374***	-0.00366***
				(-10.53)	(-7.33)	(-5.42)
Own		0.01185***	0.00049		0.01181***	0.00055
		(36.89)	(1.17)		(36.80)	(1.30)
BM			0.00084*			0.00083*
			(1.88)			(1.85)
$Cash$			0.01599***			0.01601***
			(27.12)			(27.14)
$Debt$			-0.00778***			-0.00777***
			(-17.00)			(-16.98)
$Size$			0.00793***			0.00790***
			(49.00)			(48.83)
$ExRet$			-0.01616***			-0.01670***
			(-10.70)			(-11.09)
Total Obs	427,185	400,372	354,847	427,185	400,372	354,847

Table 1.4. Fama-MacBeth Regressions of Quarterly Share Repurchase Amount on Fund Flows

This table reports Fama and MacBeth (1973) estimates examining the relationship between mutual fund flow pressure in the prior quarter and the amount of share repurchases in the current quarter. The dependent variable, $QtrRepAmt$, is defined as the maximum of zero and the negative of the percentage quarterly log change in split-adjusted shares outstanding. Columns (1)–(3) use a binary indicator of flow direction ($FlowSign$); Columns (4)–(6) use a continuous standardized flow measure ($FlowLevel$). Control variables include mutual fund ownership (Own), book-to-market ratio (BM), cash flow to debt ($Cash$), debt to assets ($Debt$), size decile ($Size$), and prior-quarter excess return ($ExRet$). All continuous independent variables –except for $ExRet$ and $Size$– are standardized each quarter to have mean zero and standard deviation one. $Size$ ranges from 1 to 10, and $ExRet$ is the firm's return minus the value-weighted market return. Reported coefficients are time-series averages of quarterly OLS estimates. Test statistics (t-values) are based on Newey and West (1987) standard errors with 4 lags. Significance levels: * p<0.10, ** p<0.05, *** p<0.01.

	$FlowSign$			$FlowLevel$		
Dependent Variable:	Quarterly Repurchase Amount					
	(1)	(2)	(3)	(4)	(5)	(6)
$FlowSign$	-0.211***	-0.158***	-0.116***			
	(-7.06)	(-6.05)	(-4.44)			
$FlowLevel$				-0.128***	-0.108***	-0.096***
				(-6.29)	(-5.32)	(-5.23)
Own		0.181***	0.004		0.185***	0.004
		(3.81)	(0.17)		(3.72)	(0.18)
BM			0.272***			0.272***
			(11.42)			(11.43)
$Cash$			0.424***			0.422***
			(8.96)			(9.00)
$Debt$			-0.111***			-0.111***
			(-7.42)			(-7.40)
$Size$			0.095***			0.096***
			(5.89)			(5.89)
$ExRet$			-0.467***			-0.461***
			(-6.90)			(-6.90)
Constant	0.603***	0.624***	0.473***	0.576***	0.582***	0.391***
	(11.64)	(11.99)	(9.78)	(15.11)	(15.11)	(9.89)
Total Obs	427,185	400,372	354,847	427,185	400,372	354,847

Table 1.5. Difference-in-Differences: Effect of Scandal Exposure on OMR Announcement Likelihood

This table presents Difference-in-Differences (DiD) estimates from pooled Probit regressions examining the effect of exposure to the 2003 mutual fund scandal on the likelihood of announcing an Open Market Repurchase (OMR) program (OMR). The sample covers the period from 2000q1 to 2006q4. OMR is a binary variable equal to 1 if a firm announces an OMR in a given quarter, and 0 otherwise. $HighlyExposed$ is a dummy equal to 1 for firms with above-median mutual fund ownership by scandal-implicated fund families in Q3 2003. $PostScandal$ equals 1 for quarters from 2003q4 onward. The table reports Average Marginal Effects (AMEs) calculated post-estimation. Column (1) includes only the DiD variables. Column (2) adds normalized mutual fund ownership (Own). Column (3) includes the full set of normalized controls: Own, BM, $Cash$, $Debt$, as well as $Size$ decile and prior-quarter excess return ($ExRet$). Column (4) adds time fixed effects (quarter dummies) and industry fixed effects to the full specification in Column (3). All control variables are lagged by one quarter. Standard errors are clustered at the firm level. Test statistics (z-values) are shown in parentheses. Significance levels: * p<0.10, ** p<0.05, *** p<0.01.

Dependent Variable:	OMR Announcement (OMR)			
	(1)	(2)	(3)	(4)
$HighlyExposed \times PostScandal$	0.01667***	0.01547***	0.01642***	0.01698***
	(4.60)	(4.50)	(5.21)	(5.35)
$HighlyExposed$	0.02104***	0.01583***	-0.00170	-0.00181
	(9.15)	(6.53)	(-0.65)	(-0.70)
$PostScandal$	0.00873***	0.00705***	0.01014***	0.00863***
	(5.41)	(4.42)	(5.37)	(4.82)
Own		0.00801***	-0.00190*	-0.00131
		(8.28)	(-1.70)	(-1.16)
BM			-0.00352***	-0.00327**
			(-2.65)	(-2.43)
$Cash$			0.01553***	0.01415***
			(10.17)	(9.55)
$Debt$			-0.01034***	-0.00933***
			(-7.47)	(-6.73)
$Size$			0.00959***	0.00789***
			(18.58)	(14.93)
$ExRet$			-0.01556***	-0.01633***
			(-4.64)	(-4.16)
Time Fixed Effects	No	No	No	Yes
Industry Fixed Effects	No	No	No	Yes
Observations	97,233	90,822	84,796	84,796

Table 1.6. Difference-in-Differences: Effect of Scandal Exposure on Quarterly Share Repurchase Amount

This table presents Difference-in-Differences (DiD) estimates from pooled OLS regressions examining the effect of exposure to the 2003 mutual fund scandal on quarterly share repurchase amounts ($QtrRepAmt$). The sample covers the period from 2000q1 to 2006q4. $HighlyExposed$ is a dummy equal to 1 for firms with above-median mutual fund ownership by scandal-implicated fund families in Q3 2003. $PostScandal$ equals 1 for quarters from 2003q4 onward. The coefficient of interest is the interaction term $HighlyExposed \times PostScandal$, which captures the differential change in repurchase behavior for the high-exposure group after the scandal. Column (1) includes only the DiD variables. Column (2) adds normalized mutual fund ownership (Own). Column (3) includes the full set of normalized controls: Own, BM, $Cash$, $Debt$, along with $Size$ decile and prior-quarter excess return ($ExRet$). Column (4) adds time fixed effects (quarter dummies) and industry fixed effects to the full specification in Column (3). All control variables are lagged by one quarter. Standard errors are clustered at the firm level. Test statistics (t-values) are shown in parentheses. Significance levels: * p<0.10, ** p<0.05, *** p<0.01.

Dependent Variable:	Quarterly Repurchase Amount			
	(1)	(2)	(3)	(4)
$HighlyExposed \times PostScandal$	0.0930***	0.0921***	0.0974***	0.1017***
	(6.60)	(6.54)	(6.41)	(6.35)
$HighlyExposed$	0.0249**	0.0152	-0.0420***	-0.0462***
	(2.16)	(1.25)	(-3.14)	(-3.39)
$PostScandal$	-0.0539***	-0.0561***	-0.0673***	-0.3258***
	(-5.71)	(-5.98)	(-6.38)	(-10.33)
Own		0.0151***	-0.0101**	-0.0085*
		(3.51)	(-2.10)	(-1.83)
BM			0.0247***	0.0182***
			(5.07)	(3.72)
$Cash$			0.0528***	0.0479***
			(14.44)	(12.55)
$Debt$			-0.0183***	-0.0164***
			(-3.38)	(-2.86)
$Size$			0.0265***	0.0204***
			(12.39)	(9.13)
$ExRet$			-0.0242***	0.0041
			(-2.93)	(0.46)
Constant	0.3932***	0.3816***	0.2823***	0.3946***
	(47.09)	(43.47)	(18.95)	(19.39)
Time Fixed Effects	No	No	No	Yes
Industry Fixed Effects	No	No	No	Yes
Observations	97,233	90,822	84,796	84,796

Table 1.7. IV First Stage: Predicting Fund Flow Sign with Scandal Exposure

This table presents the first-stage OLS regressions from the Instrumental Variable (IV) analysis, covering the period from 2003q4 to 2006q4. The dependent variable is $FlowSign$, a binary indicator equal to 1 if the firm experienced positive net fund flow in the prior quarter, and 0 otherwise. The key independent variable is the instrument, $HighlyExposed$, a dummy equal to 1 for firms with above-median mutual fund ownership by scandal-implicated fund families in Q3 2003. Column (1) includes only the instrument. Column (2) adds normalized mutual fund ownership (Own). Column (3) includes the full set of normalized controls: Own, BM, $Cash$, $Debt$, $Size$ decile, and prior-quarter excess return ($ExRet$). Column (4) adds time fixed effects (quarter dummies) and industry fixed effects to the specification in Column (3). Control variables are lagged by one quarter. $Size$ ranges from 1 to 10. $ExRet$ is defined as the firm's raw return minus the value-weighted market return. Standard errors are clustered at the firm level. Test statistics (t-values) are shown in parentheses. The reported F-statistic tests the significance of the excluded instrument ($HighlyExposed$). Significance levels: * p<0.10, ** p<0.05, *** p<0.01.

Dependent Variable:	Flow Sign ($FlowSign$)			
	(1)	(2)	(3)	(4)
$HighlyExposed$ (Instrument)	-0.07058***	-0.03886***	-0.04201***	-0.04518***
	(-10.22)	(-5.32)	(-5.19)	(-5.62)
Own		-0.04552***	-0.05230***	-0.04913***
		(-13.17)	(-13.50)	(-12.44)
BM			0.02662***	0.03558***
			(7.30)	(11.34)
$Cash$			-0.01261***	-0.00855**
			(-3.14)	(-2.01)
$Debt$			0.02173***	0.01460***
			(5.18)	(3.51)
$Size$			0.00393**	0.00612***
			(2.42)	(3.64)
$ExRet$			0.12663***	0.07451***
			(11.74)	(6.64)
Constant	0.64577***	0.68731***	0.61266***	0.39160***
	(129.53)	(124.18)	(55.34)	(25.05)
Time Fixed Effects	No	No	No	Yes
Industry Fixed Effects	No	No	No	Yes
F-statistic (Excluded Instrument)	104.45	28.30	26.94	30.47
Observations	44,956	41,632	39,213	39,213

Table 1.8. IV Probit and Probit Regressions of OMR Announcement Likelihood on Fund Flow Sign

This table presents Average Marginal Effects (AMEs) from second-stage IV Probit regressions and corresponding standard Probit models estimating the effect of prior-quarter fund flow sign ($FlowSign$) on the likelihood of announcing an Open Market Repurchase (OMR) program. The sample period is 2003q4 to 2006q4. OMR is a binary variable equal to 1 if a firm announces an OMR in a given quarter, and 0 otherwise. $FlowSign$ is a binary variable equal to 1 if the firm experienced positive net fund flow in the prior quarter, and 0 otherwise; it is treated as endogenous in the IV Probit models. The instrument for $FlowSign$ is $HighlyExposed$, a dummy equal to 1 for firms with above-median mutual fund ownership by scandal-implicated fund families in Q3 2003 (see Table 1.7). Columns (1), (3), (5), and (7) report IV Probit estimates; Columns (2), (4), (6), and (8) report corresponding Probit estimates. Columns (1)–(2) include no controls; (3)–(4) add normalized mutual fund ownership (Own); (5)–(6) add the full set of normalized controls (Own, BM, $Cash$, $Debt$), $Size$ decile, and prior-quarter excess return ($ExRet$); and (7)–(8) further include time fixed effects (quarter dummies) and industry fixed effects. All control variables are lagged one quarter. Standard errors are clustered at the firm level. Test statistics (z-values) are reported in parentheses. Significance levels: * p<0.10, ** p<0.05, *** p<0.01.

	(1) IV Probit	(2) Probit	(3) IV Probit	(4) Probit	(5) IV Probit	(6) Probit	(7) IV Probit	(8) Probit
$FlowSign$	-0.0476***	-0.0140***	-0.0512***	-0.0104***	-0.0321***	-0.0114***	-0.0338***	-0.0091***
	(-7.15)	(-6.11)	(-8.04)	(-4.55)	(-3.92)	(-3.77)	(-4.06)	(-3.11)
Own			-0.0105	0.0167***	0.0026	-0.0008	0.0027	-0.0007
			(-1.43)	(15.38)	(0.52)	(-0.56)	(0.50)	(-0.52)
BM					-0.0034	-0.0020	-0.00183	-0.0014
					(-1.28)	(-1.32)	(-0.83)	(-0.78)
$Cash$					0.0231***	0.0223***	0.01800***	0.0168***
					(9.24)	(9.55)	(7.13)	(6.40)
$Debt$					-0.0116***	-0.0103***	-0.00973***	-0.0085***
					(-4.10)	(-6.54)	(-3.25)	(-5.12)
$Size$					0.0132***	0.0132***	0.01048***	0.0101***
					(18.62)	(24.18)	(14.63)	(17.06)
$ExRet$					-0.0407***	-0.0331***	-0.04046***	-0.0312***
					(-3.27)	(-4.60)	(-3.20)	(-4.15)
Time Fixed Effects	No	No	No	No	No	No	Yes	Yes
Industry Fixed Effects	No	No	No	No	No	No	Yes	Yes
Observations	44,956	44,956	41,632	41,632	39,213	39,213	39,213	39,213

Table 1.9. IV (2SLS) and OLS Regressions of Quarterly Share Repurchase Amount on Fund Flow Sign

This table presents second-stage IV (2SLS) and corresponding OLS estimates for the effect of prior-quarter fund flow sign ($FlowSign$) on quarterly share repurchase amounts ($QtrRepAmt$). The sample period is 2003q4 to 2006q4. $QtrRepAmt$ is defined as the maximum of zero and the negative of the quarterly log change in split-adjusted shares outstanding. $FlowSign$ is a binary variable equal to 1 if the firm experienced positive net fund flow in the prior quarter, and 0 otherwise; it is treated as endogenous in the IV specifications. The instrument used in the first stage (see Table 1.7) is $HighlyExposed$, a dummy equal to 1 for firms with above-median mutual fund ownership by scandal-implicated fund families in Q3 2003. Columns (1), (3), (5), and (7) report IV (2SLS) estimates; Columns (2), (4), (6), and (8) report corresponding OLS estimates. Columns (1)–(2) include no controls; (3)–(4) add normalized mutual fund ownership (Own); (5)–(6) include the full set of normalized controls (Own, BM, $Cash$, $Debt$), $Size$ decile, and prior-quarter excess return ($ExRet$); and (7)–(8) add time fixed effects (quarter dummies) and industry fixed effects to the full specification. All control variables are lagged one quarter. Both IV and OLS regressions are estimated on the same sample within each specification pair. Standard errors are clustered at the firm level. Test statistics (t-values) are reported in parentheses. Significance levels: * p<0.10, ** p<0.05, *** p<0.01.

	(1) IV	(2) OLS	(3) IV	(4) OLS	(5) IV	(6) OLS	(7) IV	(8) OLS
$FlowSign$	-0.5706***	-0.0690***	-0.7070***	-0.0572***	-0.3139***	-0.0621***	-0.3445***	-0.0501***
	(-9.09)	(-8.72)	(-12.14)	(-7.20)	(-5.89)	(-7.25)	(-4.81)	(-3.27)
Own			-0.0645**	0.0528***	-0.0206	-0.0065	-0.0221	-0.0049
			(-2.08)	(13.76)	(-0.96)	(-1.40)	(-0.96)	(-0.95)
BM					0.0214**	0.0152***	0.0179	0.0076*
					(1.99)	(3.52)	(1.24)	(1.73)
$Cash$					0.0483***	0.0517***	0.0392***	0.0451***
					(6.97)	(10.26)	(5.98)	(7.96)
$Debt$					-0.0243**	-0.0299***	-0.0188**	-0.0230***
					(-2.22)	(-6.27)	(-1.99)	(-4.74)
$Size$					0.0430***	0.0427***	0.0351***	0.0322***
					(15.01)	(24.66)	(10.31)	(17.93)
$ExRet$					-0.0089	-0.0408**	-0.0131	-0.0366*
					(-0.19)	(-2.08)	(-0.39)	(-1.69)
Constant	0.7423***	0.4443***	0.8879***	0.3693***	0.3661	0.2114***	0.3682**	0.1357**
	(9.80)	(19.01)	(5.01)	(17.89)	(1.27)	(14.84)	(2.35)	(6.20)
Time Fixed Effects	No	No	No	No	No	No	Yes	Yes
Industry Fixed Effects	No	No	No	No	No	No	Yes	Yes
Observations	44,956	44,956	41,632	41,632	39,213	39,213	39,213	39,213

Table 1.10. Long-Run Abnormal Returns Following OMR Announcements by Prior Fund Flow Sign

This table reports Cumulative Abnormal Returns (CARs), in percent, over various horizons following Open Market Repurchase (OMR) announcements made between 1994 and 2020. The sample is split based on the sign of mutual fund flow ($FlowSign$) in the quarter preceding the announcement: $FlowSign = 0$ indicates non-positive flow, and $FlowSign = 1$ indicates positive flow. Panel A reports raw long-run CARs estimated monthly using the Fama and French (1993) three-factor model and the RATS methodology. Panel B reports long-run CARs based on excess returns of announcing firms relative to matched non-announcing firms selected first on mutual fund ownership decile and $FlowLevel$ decile (based on characteristics measured in the preceding quarter), and, where multiple candidates satisfy these conditions, further matched by industry, size decile, and book-to-market similarity. Abnormal alphas are estimated using the same RATS procedure. Reported t-statistics are based on the time-series of monthly alphas. Significance: * p<0.10, ** p<0.05, *** p<0.01.

	$FlowSign = 0$		$FlowSign = 1$	
Event Window (Months Post-Ann.)	CAR (%) (1)	t-stat (2)	CAR (%) (3)	t-stat (4)
Panel A: Long-Run CARs (Raw)				
$[1, 12]$	0.84	(0.82)	2.87**	(2.09)
$[1, 24]$	4.29**	(1.98)	4.16*	(1.92)
$[1, 36]$	10.76***	(3.27)	5.63	(1.63)
$[1, 48]$	14.33***	(3.87)	6.16	(1.49)
Panel B: Long-Run CARs (Flow-Adjusted)				
$[1, 12]$	1.28	(1.37)	-0.33	(-0.26)
$[1, 24]$	3.38*	(1.83)	-0.71	(-0.35)
$[1, 36]$	8.74***	(2.79)	2.12	(0.82)
$[1, 48]$	10.53***	(3.11)	3.36	(0.96)
Observations (Announcements)	9,983		8,399	

Table 1.11. Short-Run Announcement Effects by Prior Fund Flow Sign

This table compares short-run market reactions around Open Market Repurchase (OMR) announcements made between 1994 and 2020, splitting the sample based on the sign of mutual fund flow ($FlowSign$) in the quarter preceding the announcement. Three event-window measures are evaluated over the [-1, +2] day window relative to the announcement day (day 0): Cumulative Abnormal Return (CAR), Abnormal Volume, and Abnormal Turnover. CAR is computed as the sum of daily excess returns (stock return minus the value-weighted market return). Abnormal Volume is defined as the average log(1+Volume) during the event window minus its average over the pre-event window [-252, -1]. Abnormal Turnover is calculated analogously using log(Turnover + 0.0001275), following Bhattacharya and Jacobsen (2016). The table reports both the mean and median for each measure by flow group ($FlowSign = 0$ vs. $FlowSign = 1$). The 'Difference' columns show the gap in means and medians between groups. P-values for statistical significance are reported in brackets: within-group tests assess whether mean/median differs from zero (t-test or Wilcoxon signed-rank); between-group tests assess equality of means/medians (two-sample t-test or Wilcoxon rank-sum). Asterisks on reported values denote significance based on p-values: * p<0.10, ** p<0.05, *** p<0.01.

	$FlowSign = 0$		$FlowSign = 1$		Difference	
Measure	Mean (1)	Median (2)	Mean (3)	Median (4)	Mean (5)	Median (6)
CAR [-1, +2] (%)	1.97***	1.62***	3.12***	2.01***	1.15***	0.35***
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.001]
Abnormal Volume	0.12***	0.08**	0.16***	0.11***	0.04**	0.03***
	[0.003]	[0.011]	[0.000]	[0.001]	[0.012]	[0.003]
Abnormal Turnover	0.10**	0.08**	0.13***	0.10***	0.03**	0.02*
	[0.023]	[0.015]	[0.007]	[0.004]	[0.048]	[0.064]
Observations (Announcements)	9,983		8,399			

Chapter 2

Disagreement Resolution Horizon and Open Market Repurchase Program Completion

2.1 Introduction

Open-market share repurchase programs exhibit remarkable heterogeneity in their completion rates. Bhattacharya and Jacobsen (2016) find that 24% of all firms announcing share repurchase programs between 1985-2012 do not purchase a single share in the fiscal year of announcement. More recent evidence from my dataset of programs announced between 2004-2019 shows this pattern persists: 14% of announcing firms execute zero repurchases in the year following authorization, while at the other extreme, 10% of firms complete more than three-quarters of their announced programs within the same period. This variation is particularly intriguing because while program announcements are relatively costless and flexible, actual share repurchases require significant resource allocation, potentially diverting funds from operations and investments. The costly nature of program execution suggests that completion decisions may contain valuable information about managers' private information and intentions. Understanding what drives this substantial heterogeneity in completion rates, and what these differences signal, is therefore crucial for interpreting both announcement and execution decisions in open market repurchase programs.

The traditional view of repurchase announcements emphasizes their role as signals of undervaluation. Survey evidence from Brav et al. (2005) confirms that managers primarily initiate these programs when they perceive their stock as undervalued. Markets generally respond positively to such announcements (Vermaelen, 1981; Bartov, 1991; Comment and Jarrell, 1991), but this reaction appears incomplete. Several studies document favorable long-term returns following repurchase announcements (Ikenberry et al., 1995, 2000;

Akhigbe et al., 2007; Peyer and Vermaelen, 2009; Leng and Noronha, 2013), suggesting that the market only partially incorporates the signal's information content.

This partial market response creates a second decision point for announcing firms. If undervaluation persists after the announcement, managers may choose whether to execute costly share repurchases as a further signal. This decision is particularly important given that undervaluation can impose substantial real costs on firms. Edmans et al. (2012) show that undervalued firms face increased acquisition risk, while Baker et al. (2003) and Hau and Lai (2013) document that such firms exhibit reduced investment and employment levels. Given the costs associated with undervaluation and the flexible, non-binding nature of open market repurchase programs, managers may rationally choose to announce these programs as a low-cost signal of undervaluation. However, when this initial signal is only partially incorporated by the market and undervaluation persists, managers may then carefully weigh the costs of continued undervaluation against the more substantial costs of sending further signals through actual share repurchases.

Existing explanations for completion rate heterogeneity focus primarily on differences in the degree of undervaluation, but they offer contradictory predictions. Chan et al. (2010) suggest that low-completion firms are not actually undervalued but announce programs to mislead investors, implying these firms should underperform post-announcement. Conversely, Bhattacharya and Jacobsen (2016) argue that low-completion firms are more undervalued and can achieve price correction through announcement alone, predicting strong initial performance but no sustained outperformance. Neither explanation, however, can fully account for the patterns I document in this paper.

This paper proposes a novel explanation: the "disagreement resolution horizon hypothesis" (DRHH), and finds evidence consistent with this framework. Rather than focusing on the magnitude of undervaluation, the DRHH emphasizes the expected timeline over which manager-market disagreements about firm value are expected to resolve. The empirical patterns I document are consistent with a framework in which managers face a trade-off between two costs: the accumulating costs of continued undervaluation and the immediate costs of share repurchases. The optimal balance in this trade-off depends on how quickly managers expect their superior information to become apparent to the market through natural channels such as earnings announcements and operational developments.

Under DRHH, one would expect systematic differences in the timing of unexpected performance realizations based on completion rates. Managers who anticipate near-term resolution of disagreements choose lower completion rates, knowing their undervaluation will soon be corrected. In contrast, managers anticipating longer-horizon resolution may find it optimal to incur the immediate costs of share repurchases to signal their conviction, as the cumulative cost of extended undervaluation would otherwise be substantial.

If differences in the horizon of disagreement resolution influence managers' completion behavior and contribute to the documented completion heterogeneity, then one should observe differences in the timing of unexpected performance depending on completion rates. Specifically, relative to analyst forecasts at the time of program announcement, low-completion firms should deliver better-than-expected performance in the early years following announcement, while high-completion firms should outperform expectations in later years.

Additionally, if signals sent through program announcements or completion behavior are only partially incorporated by the market, then manager-market disagreements may not be fully resolved initially and only resolve when better-than-expected performance is actually delivered. This partial incorporation would imply that similar timing differences should be observable even relative to contemporaneous analyst forecasts, albeit with smaller magnitudes. Such patterns should also manifest in abnormal returns around earnings announcement dates, as markets react to performance surprises that persist due to incomplete signal incorporation.

To test these predictions, I construct a comprehensive dataset of repurchase programs and their completions from 2004 to 2019 using SEC Forms 10-Q and 10-K. This dataset offers significant advantages over traditional COMPUSTAT-based measures, which Banyi et al. (2008) show can deviate from actual repurchases by more than 30% in many cases. My hand-collected data allows for precise tracking of program completion rates and enables clean identification of the relationship between completion decisions and subsequent performance patterns. For each program in my sample, I measure its completion rate three months after announcement and categorize programs into four quartiles based on these completion rates, with quartile 1 representing the lowest completion and quartile 4 the highest completion.

Focusing on net income performance, I find that firms in quartile 1 (lowest completion) are significantly more likely to deliver net income above contemporaneous analyst forecasts in quarters of years one and two, while this relationship disappears in years three and four. Relative to their matched firms, these firms are 6.06% and 5.42% more likely to deliver net income above mean analyst forecasts in years one and two, respectively. In contrast, firms in quartile 4 (highest completion) show no tendency to deliver better-than-expected performance in years one and two but are significantly more likely to deliver superior performance in years three and four, with average marginal effects of 5.89% and 6.17%. These patterns are equally present when unexpected performance is measured using revenue, as documented later in the paper.

A similar and stronger pattern emerges when performance is benchmarked against analyst forecasts made at the time of program announcement. However, firms in quartile 1 are significantly more likely to deliver better-than-expected performance across all four years relative to these fixed forecasts, suggesting that analysts substantially adjusted their expectations about these firms only after observing actual superior performance in the early years. This comparison reveals that while analysts may learn from various early performance signals and other information, their expectation adjustments are incomplete, allowing disagreements to resolve gradually as performance unfolds.

Consistent with these performance timing patterns, abnormal returns around earnings announcements show that markets are genuinely surprised by these results, with low-completion firms experiencing significant positive reactions in early years and high-completion firms in later years. Finally, analysis of long-run stock returns reveals that while all announcing firms earn significant abnormal returns over four years, the timing of these returns systematically varies with completion rates, aligning with the predicted disagreement resolution horizons.

These findings cannot be explained by existing theories of completion heterogeneity. The superior early performance of low-completion firms contradicts both the notion that they announce to mislead investors and the idea that their undervaluation is fully incorporated at announcement. Instead, the evidence suggests that completion rates may serve as signals about the horizon of information asymmetry resolution, with managers strategically choosing completion levels based on their expectations about when disagreements will naturally resolve.

The empirical findings also reveal significant practical implications for investment strategies. Traditional buyback anomaly strategies rely solely on announcement signals, buying and holding all announcing firms for extended periods. While these strategies have been shown to generate abnormal returns, they fail to exploit the information content embedded in completion decisions. To test whether completion rates represent an underutilized signal, I develop a refined investment strategy that conditions on both announcement and three-month completion rates. This strategy selectively invests in low-completion firms during months 7-24 and high-completion firms during months 31-48, aligning investment timing with the predicted disagreement resolution horizons. Compared to a traditional strategy that holds all announcing firms over the same period, the refined approach delivers a statistically significant improvement of approximately 25 basis points per month in net alphas, corresponding to roughly 11% additional return over the 42-month investment window. These results suggest that completion information, which is publicly available and mechanically derivable from SEC filings, represents a practically implementable enhancement to traditional buyback-based investment strategies.

This study makes several important contributions to our understanding of corporate payout policy and information asymmetry. First, it introduces the disagreement resolution horizon hypothesis (DRHH) and provides empirical evidence consistent with the notion that completion rates reflect managers' expectations about the timeline of information asymmetry resolution rather than merely the degree of undervaluation. Second, it provides novel evidence suggesting that managers may strategically balance the temporal distribution of costs - weighing the duration-dependent costs of undervaluation against the immediate costs of share repurchases. Third, it shows how completion rates can serve as valuable signals to market participants, not only about the existence of undervaluation but also about its expected resolution horizon. Finally, by highlighting the importance of temporal dynamics in information asymmetry, this paper extends traditional signaling theories that have traditionally focused on the magnitude rather than the duration of information gaps between managers and markets.

2.2 Data

2.2.1 Quarterly repurchase and Repurchase announcements

The Data Gap in Repurchase Literature

Traditional repurchase research has relied on two separate datasets that could not be linked: COMPUSTAT, which provides quarterly aggregate repurchase figures, and SDC, which contains repurchase program announcements. COMPUSTAT tells us how many shares a firm repurchased in a given quarter but provides no information about which specific programs these repurchases were conducted under. Conversely, SDC provides announcement dates and program sizes but offers little insight into program completion rates over time. This disconnection has limited researchers' ability to analyze the relationship between announced programs and actual execution. Beyond this fundamental connectivity issue, the accuracy of commonly used repurchase measures has been questioned. Banyi et al. (2008), using a hand-collected sample of 958 firms, compared actual share repurchases reported in SEC filings to CRSP and COMPUSTAT-based estimates. They found that while COMPUSTAT provides the most accurate measure among existing databases, it still deviates from actual repurchases by more than 30% in approximately 16% of cases. The authors concluded that "many studies should be revisited now that the SEC mandates disclosure of precise information about share repurchases in Forms 10-Q and 10-K" and encouraged future research to utilize SEC filing data directly.

Despite this call for improved data collection, relatively few studies have undertaken the substantial effort required to hand-collect comprehensive datasets from SEC filings. Hillert et al. (2016) represent one of the few exceptions, constructing a program-level completion dataset covering 2004-2010. Building on their pioneering approach, I construct what is, to the best of my knowledge, the most comprehensive repurchase dataset built on SEC filings, covering the period 2004-2019 and nearly doubling the temporal scope of prior efforts.

Data Collection Methodology

Beginning March 15, 2004, SEC regulations required firms to disclose monthly repurchase activity in Forms 10-Q (Item 2(e)) and 10-K (Item 5(c)). These disclosures must include four key elements: the number of shares repurchased, average purchase price, shares bought under publicly announced programs, and remaining program capacity (in dollars or shares).

I constructed my sample using CRSP data for common stocks (codes 10-11) listed on major U.S. exchanges (NYSE, Amex, Nasdaq) from January 2004 through December 2019. Using automated scripts, I extracted monthly repurchase information from SEC filings (Forms 10-Q and 10-K) for all firms in this universe.

The innovation of my data collection approach lies in extracting not only the tabular repurchase data but also analyzing the surrounding text through natural language processing (NLP). This text analysis allows me to link specific monthly and quarterly repurchase figures to individual repurchase programs—effectively bridging the gap between aggregate repurchase data and program-level information identified by earlier research. Due to non-adherence to the proposed disclosure format by many firms, I manually reviewed and corrected such entries. This initial data collection process yielded 11,231 repurchase programs. I subsequently applied several filters to ensure data quality and completeness. First, I excluded firms for which I could not locate accounting data on COMPUSTAT or mutual fund ownership information on the CDA/Spectrum mutual fund database for the quarter prior to the announcement. Second, I excluded firms lacking analyst data on IBES for the four years following the announcement. Finally, I further excluded programs that were not open market repurchases, lacked a stated authorization date, or did not specify a fixed program size. After applying these filters, the final dataset comprised 10,112 programs across 3,890 firms with at least one announcement during the sample period.

Data Validation Against COMPUSTAT

To validate the accuracy of my hand-collected data, I compared my quarterly repurchase figures with those reported in COMPUSTAT. This comparison focuses on the total number of shares repurchased (the sum across all programs and other repurchase activities), as this is the only comparable metric available in both datasets. COMPUSTAT provides only aggregate totals and does not detail program-specific repurchases or remaining program balances—information that is central to my analysis but unavailable in existing databases. The validation reveals strong concordance: 95% of my quarterly repurchase figures are within 5% of their COMPUSTAT counterparts. This minor discrepancy generally arises because COMPUSTAT reports figures in millions of shares and applies rounding. For the remaining 5% of cases where differences exceed 5%, I observed two predominant patterns: either COMPUSTAT reports zero repurchases while my dataset shows nonzero figures, or the figures differ by three orders of magnitude. Random verification of 20% of such cases revealed that my dataset is almost invariably accurate. The common error stems from COMPUSTAT's misinterpretation of reporting units. For example, when a company reports quarterly repurchases as ”1,200 thousand shares,” COMPUSTAT might extract this as merely ”1,200 shares” and erroneously round it to zero. When the unit is incorrectly extracted but not rounded to zero, a three-order magnitude difference typically emerges.

Limitations of COMPUSTAT Repurchase Data

This comparison reveals significant concerns about COMPUSTAT's quarterly repurchase data quality. While only 5% of figures significantly deviate, the magnitude of errors is substantial. COMPUSTAT figures tend to be smaller by orders of magnitude, which is particularly problematic because these erroneous small figures may not be detected as outliers and excluded from analyses, potentially skewing research results. Beyond accuracy issues, COMPUSTAT's aggregate measure differs fundamentally from program-specific repurchases. The total shares repurchased encompasses various transactions including: shares returned for tax payments on vested restricted stock units, shares surrendered by employees for tax liabilities and stock option exercises, and repurchases of unvested restricted stock from terminated employees. In these scenarios, employees—not the company—decide on share transactions, resembling insider selling more than corporate repurchasing. Moreover, acquisition prices may deviate from market prices. This distinction has practical research implications. Studies that classify firms as ”repurchasers” versus ”non-repurchasers” based on any share repurchase activity (Leng and Noronha, 2013; Bhattacharya and Jacobsen, 2016; Yook, 2010) would misclassify 19% of true non-repurchasers as repurchasers in the first quarter following announcements when using COMPUSTAT data instead of program-specific figures

Program Completion Measures

The unique advantage of my dataset lies in providing quarterly figures for remaining amounts under specific repurchase programs. Through extensive text analysis, I link program details with remaining balance disclosures, enabling calculation of completion rates as: (Initial Program Size - Current Remaining Balance) / Initial Program Size. Table 2.1 presents completion level summary statistics following program initiation. Mean completion levels are 14.04%, 41.46%, 50.84%, and 57.01% at three months, one year, two years, and three years after initiation, respectively. These completion rates are broadly consistent with those reported by Hillert et al. (2016), who found average completion rates of 45.53%, 53.17%, and 59.31% at one, two, and three years after program initiation, respectively, providing external validation of my data collection methodology.

The data reveal substantial heterogeneity in program completion rates, underscoring a key motivation for this study. One year after authorization, the bottom quartile of firms has completed less than 15% of their programs, while the top quartile has completed over 62%. The dispersion is even more pronounced at the extremes: 10% of firms have not repurchased a single share after a full year, while another 10% have completed more than three-quarters of their programs. This remarkable variation in execution behavior highlights the importance of understanding what drives completion decisions and what these differences signal about firm characteristics and managerial intentions.

2.2.2 Other Data

Stock return and trading information are obtained from the CRSP daily stock file. Firm-level accounting variables and financial ratios are sourced from the quarterly COMPUSTAT files, while analyst coverage data come from the Institutional Brokers' Estimate System (IBES). Quarterly mutual fund ownership data are obtained from the CDA/Spectrum mutual fund database.

Using these datasets, I construct several firm characteristics that later serve as control variables in the empirical analysis. $Size$ is the market capitalization decile, computed each quarter by sorting all common stocks listed on the NYSE, NASDAQ, and AMEX by their market capitalization (price times shares outstanding) and ranking them into ten groups. Own is the mutual fund ownership decile, constructed analogously by sorting firms on the fraction of shares outstanding held by mutual funds. $Debt$ is the leverage ratio, defined as total debt divided by total assets. $Cash$ is operating cash flow scaled by total debt. ROA is operating income before depreciation divided by total assets. For analyst coverage, I report the number of analysts providing earnings forecasts in Table 2.2. In the regression analysis, however, I use the log-transformed measure, $Analysts = \log(1 + \text{number of analysts})$, to reduce skewness and mitigate the influence of outliers.

Because the empirical analysis examines how firm behavior varies across repurchase program completion levels, I report firm characteristics across quartiles of three-month completion rates in Table 2.2. Specifically, I sort firms into four groups based on the fraction of the authorized program completed within the first three months after announcement (with quartile one corresponding to the lowest completion rates and quartile four to the highest). All firm characteristics are measured in the quarter prior to announcement. High-completion firms (quartile four) exhibit greater analyst coverage, lower leverage ratios ($Debt$), higher cash flow ratios ($Cash$), and slightly larger firm size ($Size$) compared to low-completion firms (quartile one), suggesting that these firms tend to be less financially constrained and enjoy greater market visibility. This combination of stronger information environments and improved financial flexibility may reduce the cost of executing repurchases, making higher completion more feasible.

Table 2.2 also reports differences in program size. I define $ProgramSize$ as the number of shares authorized divided by the number of shares outstanding at announcement. While quartile one displays the highest mean program size (7.711%), quartile four has a higher median program size (5.875% vs. 4.916%), indicating that high-completion firms are not simply those announcing small, easy-to-execute programs. Instead, the observed variation in completion behavior reflects meaningful differences in execution commitment across firms with different financial and informational characteristics.

2.3 Empirical Results

2.3.1 Repurchase announcement and unexpected performance

This section examines the relative performance of announcing firms compared to analysts' forecasts in the four years following a repurchase announcement. The focus is on quarterly net income and revenue for two primary reasons. First, these measures are the most widely covered by analysts and are readily available in the IBES database. Second, they are not influenced by changes in the number of shares outstanding, which repurchases directly affect, thus impacting per-share figures such as EPS.

For each firm-quarter, I define a dummy variable $Beat$ equal to one if the firm's net income (or revenue) exceeds the mean analysts' forecast in that quarter, and zero otherwise. Next, I match each announcing firm with a similar non-announcing firm based on industry and the number of analysts covering the firm. Specifically, for each announcing firm, I select a non-announcing firm with the same 2-digit SIC code and the closest number of analyst coverage in the quarter prior to the announcement. If multiple firms meet these criteria, I select the one with the closest book-to-market ratio to the announcing firm. The matching procedure is based on values from the quarter preceding the repurchase announcement[1].

Subsequently, I pool four quarters within a year and estimate the following probit regression for each year (year 1 to year 4) after the announcement:

$$Beat_{i,t} = a_y + b_y * Announce_i + \sum_{k=1}^{n} b_{y,k} * Control_{i,k} + \epsilon_{i,t} \tag{2.1}$$

where i denotes the firm, t the fiscal quarter, and $y \in \{1, 2, 3, 4\}$ the event year after the announcement. Year 1 is the *first year following the announcement*, excluding the first calendar quarter (months 1–3 after month 0, the announcement month). Said differently, Year 1 spans months $[+4, +12]$; Year 2 spans $[+13, +24]$; Year 3 spans $[+25, +36]$; and Year 4 spans $[+37, +48]$. I exclude the first three months of Year 1 for comparability with the setup in the next subsection. $Beat_{i,t}$ equals one if firm i's net income (or revenue) in quarter t exceeds the mean analyst forecast for that quarter, and zero otherwise. $Announce_i$ equals one for the announcing firm in each matched pair and zero for its matched firm.

[1] To validate this matching procedure, I compare the mean and median of size decile, mutual fund ownership decile, book-to-market ratio, leverage ratio, and cash ratio between matched firms and announcing firms. Even at a 10% significance level, there are no significant differences between these two groups.

All specifications include *calendar-quarter* fixed effects to absorb aggregate time shocks, and standard errors are clustered at the firm level.

While the matched firm is similar along many dimensions, I include controls to absorb any residual differences relevant for beating analysts' expectations. The control vector comprises mutual fund ownership decile (Own), size decile ($Size$), analyst coverage ($Analysts$), book-to-market ratio (BM), leverage ($Debt$), return on assets (ROA), and a cash-flow measure ($Cash$). I measure all controls in the quarter immediately prior to the announcement (quarter -1) and hold them fixed in post-announcement regressions to avoid conditioning on post-treatment variables.

The two decile variables are constructed cross-sectionally each quarter. $Size$ is the within-quarter decile rank of market capitalization ($PRC \times SHROUT$) among CRSP common shares listed on NYSE/NASDAQ/AMEX. Own is the within-quarter decile rank of mutual-fund ownership (CDA/Spectrum), computed as mutual-fund–held shares divided by shares outstanding and then ranked among the same universe. Using deciles mitigates extreme skewness (especially for size), improves comparability over time, and dampens artificial time-series jumps in raw mutual-fund holdings that arise from coverage changes in CDA/Spectrum.

The remaining controls are defined as follows. $Analysts$ is the log of one plus the number of analysts covering firm; BM is book equity over market equity; $Debt$ is total liabilities over total assets; ROA is operating income before depreciation divided by total assets; and $Cash$ is operating cash flow scaled by total debt. I include $Size$, Own, and $Analysts$ to capture firm visibility and the information environment, and BM, $Cash$, and $Debt$ to capture valuation, cash-generation capacity, and financial constraints that are known to covary with forecast errors and beat/miss probabilities. All control variables are calculated using values from the quarter prior to the announcement.

Tables 2.3 and 2.4 report the results of this regression for net income and revenue, respectively. Focusing first on Table 2.3, the average marginal effect (AME) of the *Announce* variable is positive and statistically significant across all four years: at the 10% level in year one, at the 5% level in years two and four, and at the 1% level in year three. These AMEs represent modestly significant economic effects of 2.52%, 3.37%, 3.94%, and 2.78% for years one through four, respectively. To gauge economic significance, the baseline probability of beating analyst forecasts for matched firms ranges between 50.9% and 51.6% across the four years. The largest marginal effect of 3.94% in year three therefore represents approximately a 7.7% increase over this baseline (3.94/51.1), indicating that announcing firms are nearly 4 percentage points more likely to exceed analyst net income forecasts compared to their matched counterparts.

Table 2.4, which focuses on revenue performance, shows a similar pattern to Table 2.3. The average marginal effect of the announce variable is positive for all years and statistically significant in years two through four. These results indicate that announcing firms are more likely to beat analyst revenue forecasts in the years following the announcement compared to their matched firms. The revenue results provide additional insight beyond the net income findings. While superior net income performance could potentially reflect post-announcement operational improvements or cost-cutting measures that analysts failed to anticipate, the superior revenue performance may suggest that managers possessed different views about fundamental business prospects at the time of announcement. This distinction may be important because revenue is less susceptible to managerial actions following the announcement, potentially making it a cleaner test of ex-ante information differences

Research on whether announcing firms experience improvements in operating performance following the announcement of open market repurchase programs has not converged to a unified conclusion. Grullon and Michaely (2004) find that announcements of open-market share repurchase programs are not followed by an increase in operating performance, while Lie (2005) documents that operating performance improves following such announcements. These conflicting findings are particularly perplexing considering that both papers use quite similar samples of announcements. My methodology in Tables 2.3 and 2.4 differs from those used in previous papers focusing on operating performance improvement, making direct comparisons challenging. However, a comparison between these methodologies and their respective advantages and limitations can be informative.

The common strategy used in prior papers focusing on operating performance improvement is to calculate the difference in operating performance in subsequent years relative to the firm's performance at the time of announcement, adjusted by the change over the same interval in matched firms to offset any expected change. In contrast, I rely on the consensus of analysts to define surprising subsequent performance and use matched firms to address constant biases and time trend dynamics in analyst consensus. This approach offers several advantages. Since prior papers rely fully on the matching firm method to offset any expected changes, their results are highly sensitive to the choice of matching firms. Lie (2005) argues that using a different matching strategy is one reason his results differ from prior papers. Moreover, even if matching-firm adjusted change in operating performance correctly captures surprising performance change with regard to the information available at the time of announcement, it does not provide insights about the evolution of information asymmetry following the announcement over time. After a repurchase is announced and the market at least partially incorporates this information, it remains unclear whether actual performance delivered in subsequent quarters continues to surprise the market at the time those performance measures are reported. My method allows for the study of relative quarterly performance of the firm with respect to the current consensus in that quarter, thus capturing the dynamics of surprising performance. This feature is especially useful in understanding the nature of long-run stock performance following stock repurchases. Consistent with the persistent undervaluation documented in prior studies, Tables 2.3 and 2.4 reveal that firm performance is also being underestimated persistently.

2.3.2 Completion and unexpected performance

Having established that announcing firms generally outperform analyst forecasts, I now examine how this outperformance varies with program completion rates. DRHH predicts that firms with different completion rates should exhibit different temporal patterns in their outperformance. To test this prediction, I divide announcing firms into quartiles based on their completion rates three months after the announcement and estimate equation (2.1) separately for each quartile, using the same control variables, calendar-quarter fixed effects, and firm-level clustered standard errors as in the previous analysis. As in the previous section, the dependent variable is a dummy equal to one if the firm's quarterly performance exceeds the mean analyst forecast.

The choice of a three-month completion threshold requires careful justification, as it is central to studying whether completion behavior is affected by managers' beliefs regarding the horizon over which their disagreement with the market will resolve. The threshold must balance two competing considerations. On one hand, using too short a window (e.g., one month) introduces substantial noise, as completion behavior over such brief periods may not meaningfully distinguish firms' commitment levels. On the other hand, using longer thresholds (e.g., 12 months) introduces a different problem: completion differences may increasingly reflect managerial reactions to information that has already been revealed rather than ex-ante beliefs about future information arrival. Under DRHH, managers' initial completion decisions should reflect their private knowledge about when disagreement with the market will naturally resolve through the unfolding of business developments. A three-month window is sufficiently long to capture meaningful completion differences while being short enough that these differences more likely reflect managers' initial expectations rather than adaptive responses to interim information revelation. Additionally, no firm in my sample achieves full completion within three months, ensuring that the completion measure meaningfully distinguishes across firms without being truncated by the 100% completion bound.

Tables 2.5 and 2.6 reveal striking differences in the timing of outperformance between firms with low and high completion rates. Firms in the bottom quartile of completion (quartile 1) are significantly more likely to exceed analyst forecasts for both net income and revenue in the first two years following the announcement, but show no significant outperformance in years three and four. In contrast, firms in the top quartile of completion (quartile 4) display the opposite pattern: they show no significant outperformance in the first two years but are significantly more likely to exceed analyst forecasts in years three and four.

While Tables 2.5 and 2.6 demonstrate clear temporal patterns in performance relative to contemporaneous analyst forecasts, they don't directly establish whether these patterns were anticipated at the time of the repurchase announcement. If the announcement of the repurchase program and subsequent 3-month completion behavior reflect managers' disagreement with market expectations about firm prospects and their beliefs about the horizon over which these disagreements will naturally resolve, then a more direct test would examine performance surprises relative to analyst forecasts available at the time of announcement. This approach would better capture whether managers possessed superior information about the timing of future performance improvements when they made their initial completion decisions. To address this question, I examine performance relative to analyst forecasts made at the announcement time. However, this analysis presents two challenges. First, analysts rarely provide quarterly forecasts beyond the immediate future, especially for years three and four. Second, forecasts for more distant periods are typically provided only for fiscal year-end results, creating potential timing misalignment with repurchase announcements.

To handle these challenges while maintaining the integrity of the analysis, I focus on programs announced within one month of the firm's fiscal year-end. For example, if a firm's fiscal year ends in December, I include only programs announced in November, December, or January. This restriction ensures that the timing of analyst estimates aligns consistently with post-announcement periods across all firms in the sample. While this approach reduces the sample size, it provides cleaner identification of the relationship between completion rates and expected performance at the announcement time.

Tables 2.7 and 2.8 present results using fiscal year performance against announcement-time analyst forecasts. Since the patterns are similar for both net income and revenue, I focus the discussion on net income results by comparing Table 2.7 with Table 2.5. This comparison reveals important insights about how analyst expectations evolve and how disagreement resolution unfolds over time.

For firms in the lowest completion quartile (quartile 1), both tables show significant outperformance in years one and two, with notably larger magnitudes when using announcement-time forecasts (7.57% and 8.71% versus 6.06% and 5.42%). However, the key difference emerges in years three and four. While these firms show no significant outperformance relative to contemporaneous forecasts in later years, they continue to significantly beat announcement-time forecasts (6.75% and 5.74%). This pattern is consistent with the interpretation that the superior performance in years one and two was not temporary but rather persistent. The disappearance of outperformance in Table 2.5 for years three and four reflects analyst learning and expectation adjustment rather than deteriorating firm performance. After observing strong early performance, analysts revised their forecasts upward, eliminating future surprises relative to contemporaneous expectations while the underlying performance advantages persisted.

For firms in the highest completion quartile (quartile 4), the comparison reveals a different dynamic. These firms show significant outperformance only in years three and four in both tables, with larger magnitudes against announcement-time forecasts (9.32% and 11.98% versus 5.89% and 6.17%). The absence of early outperformance in both specifications indicates that no significant disagreement resolution occurred in the first two years. Consequently, when superior performance finally materialized in years three and four, it continued to substantially surprise analysts even relative to contemporaneous forecasts, though the smaller magnitudes suggest analysts had partially adjusted their expectations upward for these firms.

These patterns are consistent with the disagreement resolution horizon hypothesis. The comparison suggests that managers' decisions regarding announcement of repurchase programs and subsequent completion behavior reflect their disagreement with the market about firm prospects and the horizon over which they believe such disagreement will naturally resolve. These patterns also suggest that while analysts adjust their expectations in the right direction over time, such correction is only partial and a significant part of the disagreement resolves when better-than-expected performance is actually observed.

2.3.3 Completion and Earnings Announcement Abnormal Returns

While the analysis of analyst forecasts demonstrates that firms with the lowest completion rates deliver positively surprising performance in the first two years after announcement, and firms with the highest completion rates deliver positively surprising performance in years three and four, one might question whether these results truly represent market surprises. Two considerations warrant caution about the previous findings. First, my dependent variable (*Beat*) is a simple indicator equal to one when net income (or revenue) exceeds the mean analyst forecast for that quarter; it does not capture the magnitude of the surprise. Second, because analysts can revise their forecasts with a lag, a surprise relative to consensus may not necessarily imply that investors are contemporaneously surprised.

To address these concerns and validate that the documented post-announcement earnings patterns also surprise the market, I examine stock price reactions around earnings announcements. Specifically, I compute cumulative abnormal returns (CARs) over a four-day window $[-1,+2]$ around each quarterly earnings announcement. For each firm-earnings announcement pair, I estimate a one-factor market model using the equal-weighted market index over a pre-event estimation window spanning trading days $[-250,-10]$. Abnormal returns are calculated as the realized daily returns minus the model-implied returns in the event window; the CAR is the sum of these four abnormal returns. Consistent with measuring the completion signal over months 0–3 after authorization, I exclude the first calendar quarter of event year 1 from all post-announcement analyses.

I then summarize CARs within the 16 groups defined by completion quartile (1–4) and post-announcement year (1–4). To account for the clustering of earnings announcements on common dates, mean CARs in each group are estimated as the intercept from a constant-only regression with standard errors clustered by earnings announcement date. For median CARs, I first compute the calendar-month median CAR within each group and then test the mean of that monthly time series using a Newey-West HAC regression (lag 5), which accounts for heteroskedasticity and low-order serial correlation.

The results, reported in Table 2.9, mirror the analyst-based patterns documented earlier. Panel A (means) shows that firms in the bottom completion quartile exhibit significant positive announcement-window returns in years 1–2, with no detectable price reaction in years 3–4. In contrast, firms in the top completion quartile display little reaction in the early years but significant positive CARs in years 3–4. Panel B (medians)—based on monthly medians with HAC inference—shows the same temporal pattern. These findings indicate that the surprises documented relative to analyst forecasts translate into immediate price reactions, providing market-based validation consistent with the disagreement resolution horizon hypothesis.

These findings provide an interesting comparison to Lie (2005), who categorizes announcing firms into three groups based on COMPUSTAT data: non-repurchasers (firms that did not repurchase during the announcement quarter), repurchasers (firms that repurchased shares exceeding 1% of market value during the announcement quarter), and others. Focusing on repurchasers and non-repurchasers, Lie (2005) calculates cumulative abnormal returns around earnings announcements over eight subsequent quarters following the repurchase announcement. These eight quarters correspond to years one and two in my analysis, with his non-repurchaser group roughly corresponding to my bottom completion quartile.

The mean cumulative abnormal returns I document for the bottom quartile in years one and two are notably larger in magnitude and more statistically significant than those reported by Lie (2005). While he finds positive abnormal returns for non-repurchasers across all eight quarters, these returns are generally not significant at conventional levels.

This difference likely reflects distinct sample periods and disclosure regimes. Lie (2005) examines the period 1981–2000, whereas my sample spans 2004–2019, beginning after enhanced SEC repurchase disclosure requirements took effect in 2004. Bonaimé (2015) documents meaningful changes in firms' repurchase behavior following these disclosure mandates: firms announce fewer and slightly smaller open-market programs, while completion rates increase—consistent with a decline in opportunistic or false signaling. In this improved disclosure environment, fewer firms are likely to announce repurchases opportunistically, making low completion in my sample period less likely to reflect false signaling than in Lie (2005)'s earlier sample. This institutional change may help account for why the performance surprises I document for low-completion firms are more pronounced and statistically significant.

2.3.4 Completion and Long Run Stock Price Performance

Long-run abnormal stock price performance following repurchase announcements is well documented. Ikenberry et al. (1995), examining announcements between 1980 and 1990, report average abnormal buy-and-hold returns of 12.1% over the four years after announcement, attributing the pattern to market underreaction—a result also observed for self-tender offers (Lakonishok and Vermaelen, 1990). More recent evidence in Peyer and Vermaelen (2009) and Leng and Noronha (2013) confirms the persistence of this abnormal performance in later samples.

Building on these findings, I use program-level completion to test whether the timing of long-run abnormal returns varies with completion rates. Motivated by the earlier evidence that performance surprises arrive earlier for low completers and later for high completers, I examine year-by-year abnormal returns using two approaches. First, following Grullon and Michaely (2004) and Leng and Noronha (2013), I estimate daily Carhart four-factor regressions for each announcing firm in each event year (1–4); the regression intercept represents that firm's daily abnormal return (alpha) for the event year. Table 2.10 reports mean and median alphas by completion quartile for each post-announcement year and for the full four-year window (excluding the first three months). To account for heteroskedasticity and low-order serial correlation in returns and as a complementary design, I form calendar-time portfolios for each completion-quartile × event-year group and estimate monthly alphas. Table 2.11 reports the resulting calendar-time alphas by completion quartile and event year, along with quartile-level alphas pooling months 4–48.

Tables 2.10 and 2.11 show qualitatively similar results and reveal several key patterns. First, significant long-run abnormal performance exists across all completion quartiles, confirming that the documented post-announcement drift is a broader phenomenon not limited to specific completion levels. Notably, the magnitude of abnormal performance appears largest in quartiles one and four, indicating that firms with the lowest and highest completion rates in the first three months after announcement experience the highest degree of long-run abnormal performance. Second, and more importantly, the timing of abnormal returns varies systematically with completion rates in a manner consistent with my earlier findings.

Firms in the bottom completion quartile (quartile 1) realize the majority of their abnormal returns in years one and two, with statistically significant and economically meaningful alphas in these years but insignificant returns in years three and four. In contrast, firms in the top completion quartile show their strongest performance in years three and four, where the bulk of their abnormal returns occurs. Monthly alphas for these firms in years three and four (0.5437% and 0.5878%) are roughly twice those in years one and two (0.3036% and 0.3412%). Importantly, while these high-completion firms realize most of their abnormal performance in later years, they do achieve positive and significant returns in years one and two. The presence of these early returns, while modest compared to their later performance, suggests that the market may partially incorporate the signal conveyed by their intensive share repurchases.

These patterns are consistent with the earlier evidence. The timing of abnormal returns aligns with when firms in different completion quartiles deliver superior operating performance, suggesting that completion rates may signal the horizon over which manager-market disagreements will resolve. Moreover, these findings suggest that completion rates could be used to enhance traditional post-announcement investment strategies. While prior research documents significant abnormal returns from buying and holding all announcing firms, my results indicate that investors might achieve better timing by conditioning their investments on completion rates. Specifically, a refined strategy might focus on firms with the lowest and highest completion rates, as they show the highest abnormal performance over the next four years. Additionally, the significant difference in the timing of abnormal performance between these two quartiles suggests immediately investing in firms with low three-month completion rates (holding for years one and two) while delaying investment in high-completion firms until year three (holding through year four). Such an approach would better align investment horizons with the temporal pattern of abnormal returns, potentially improving upon the performance of simpler buy-and-hold strategies.

To test this proposition, I implement a simple, tradable refinement to the classic buyback strategy and compare its performance directly. The *Classic* strategy relies only on the announcement signal and, each month, holds an equal-weighted portfolio of all firms with authorizations in the prior 7–48 months. The *Refined* strategy conditions on the early completion signal: in each calendar month it holds the union of (i) low-completion (quartile 1) authorizations in months 7–24 and (ii) high-completion (quartile 4) authorizations in months 31–48. Both strategies begin in month 7 so that the three-month completion measure is observable after EDGAR filing lags. In my data, the lag between the quarter-end period report date and the SEC filing date is at most 63 days (99th percentile: 46 days). Starting at month 7 therefore ensures implementability for all observations. Although the *Classic* strategy can be initiated immediately at announcement, I also start it at Month 7 to provide an apples-to-apples comparison over an identical trading window.

I evaluate the two strategies using standard calendar-time factor regressions on monthly equal-weighted returns. Specifically, I estimate Carhart four-factor models and report alphas in percentage points per month, with Newey-West (1987) HAC standard errors (lag 6). To quantify trading frictions, I compute average one-way monthly turnover for each strategy and report net alphas by subtracting $c \times \text{TO}_t$ each month with $c = 10$ basis points.

Table 2.12 shows that the Refined strategy delivers a substantially larger gross alpha than the Classic strategy over the same tradable window (months 7–48): 0.5529% versus 0.2992% per month, respectively. The difference portfolio (Refined minus Classic) has a gross alpha of 0.2536% per month ($p = 0.036$). The Refined strategy's higher average turnover (8.75% versus 4.77% one-way per month) implies only a modest incremental cost under a 10 basis point assumption, so net alphas remain essentially unchanged: 0.5441% (Refined) versus 0.2944% (Classic), with a net alpha difference of 0.2497% per month ($p = 0.038$).

Economically, a net alpha gap of 0.2497% per month sustained over the 42-month window corresponds to roughly 10.5 percentage points on a simple basis, or approximately 11% on a compounded basis. Thus, conditioning on early completion produces a materially higher and statistically significant improvement relative to the classic post-announcement strategy, even after conservative trading-cost adjustments. Since completion information is public and mechanically derivable from timely filings, these results indicate that the three-month completion signal represents an implementable and underutilized enhancement to traditional buyback-based strategies.

2.4 Summary and Conclusion

Open market repurchase programs have become the dominant form of corporate payout policy over the past four decades. Despite their widespread adoption, a puzzling feature of these programs is the substantial heterogeneity in their completion rates. Using a comprehensive dataset of repurchase programs from 2004 to 2019, I document that over 14% of announcing firms have zero completion one year after authorization, while others complete their programs rapidly. This variation is particularly intriguing because while announcements are relatively costless, actual share repurchases involve significant resource allocation, suggesting that completion decisions may contain valuable information about managers' beliefs and intentions.

Prior literature has attempted to explain this heterogeneity primarily through differences in the degree of undervaluation. One view suggests that low-completion firms are not truly undervalued but announce programs to mislead investors. An alternative perspective argues that these firms are actually more undervalued but can achieve price correction through announcement alone, making actual repurchases unnecessary. However, my empirical findings challenge both explanations. I find that both low and high-completion firms deliver superior performance relative to expectations, but crucially, they do so over different time horizons.

This paper proposes and tests the "disagreement resolution horizon hypothesis" (DRHH) to explain these patterns. The DRHH argues that completion rates reflect managers' expectations about when their disagreement with the market will naturally resolve. Managers who anticipate near-term resolution of disagreement may limit costly share repurchases, knowing their superior performance will soon become apparent. Conversely, managers expecting longer-horizon resolution may find it optimal to incur repurchase costs to signal their conviction, as the costs of extended undervaluation would otherwise be substantial.

Three distinct empirical analyses support this hypothesis. First, firms in the lowest completion quartile significantly outperform analyst expectations in years one and two post-announcement, while high-completion firms show superior performance in years three and four. Second, this pattern is reflected in market reactions, with low-completion firms experiencing significant positive returns around earnings announcements in early years and high-completion firms in later years. Finally, analysis of long-run stock returns reveals that while all announcing firms earn significant abnormal returns over the four-year post-announcement period, the timing of these returns systematically varies with completion rates in a manner consistent with the DRHH.

These findings have important implications for corporate finance theory and practice. First, they suggest that the information content of repurchase programs extends beyond the simple announcement effect, with completion rates providing valuable signals about the horizon of information asymmetry resolution. Second, they highlight how managers strategically balance the costs of undervaluation against the costs of actual repurchases, with the expected duration of disagreement playing a crucial role. Third, they suggest potential improvements to post-announcement investment strategies, as completion rates might help predict the timing of abnormal returns.

More broadly, this paper contributes to our understanding of corporate signaling mechanisms by highlighting the importance of temporal dynamics in information asymmetry resolution. While previous research has focused primarily on the degree of information asymmetry, these findings suggest that the expected duration of such asymmetry can significantly influence both corporate decisions and market outcomes. Future research might explore whether similar temporal considerations affect other corporate decisions where managers possess superior information about future prospects.

2.5 Tables

Table 2.1. Summary Statistics for Repurchase Program Completion Rates

This table reports summary statistics for completion rates of open market repurchase programs at various intervals following program authorization. The sample consists of all open market repurchase programs announced between 2004 and 2019 by firms listed on NYSE, NASDAQ, and AMEX. Completion rates are calculated using data from SEC Forms 10-Q and 10-K, where completion rate is defined as the difference between the initial program size and the remaining authorization balance, divided by the initial program size, expressed as a percentage. For each time horizon (3, 12, 24, and 36 months after authorization), the table presents the mean, median, and various percentiles of completion rates across all programs active during that period. A completion rate of zero indicates no shares were repurchased during the period, while a rate of 100 would indicate full program completion.

	Mean	Median	10%	25%	75%	90%
3 Months After Authorization	14.04	12.11	0.00	2.65	20.86	26.97
12 Months After Authorization	41.46	39.53	0.00	15.34	62.65	76.32
24 Months After Authorization	50.84	48.59	12.91	33.24	72.96	87.49
36 Months After Authorization	57.01	55.76	14.61	39.91	78.61	91.27

Table 2.2. Firm Characteristics by Repurchase Program Completion Quartiles

This table presents firm characteristics across quartiles of repurchase program completion rates for open market repurchase programs announced between 2004 and 2019. Firms are sorted into quartiles based on their program completion rate three months after the announcement, where completion rate is defined as the difference between the initial program size and the remaining authorization balance, divided by the initial program size. Quartile 1 represents the lowest completion rates and Quartile 4 the highest. The full sample statistics are also reported. For each characteristic, the table reports the mean (top row) and median (in parentheses). *Size* is constructed by sorting all NYSE, NASDAQ, and AMEX common stocks based on market capitalization in the quarter prior to announcement. *Own* is similarly constructed based on the percentage of shares owned by mutual funds in the prior quarter. *Analysts* is the number of analysts covering the firm in the quarter prior to announcement. *BM* ratio is the ratio of book value of equity to market value of equity. *Debt* is total debt divided by total book value of assets. *Cash* is operating cash flow as a fraction of total debt. *ROA* is operating income before depreciation divided by total assets. *ProgramSize* is calculated as the fraction of number of shares announced on the authorization date to the number of shares outstanding. All characteristics are measured in the quarter prior to the repurchase announcement.

Characteristic	Quartile 1	Quartile 2	Quartile 3	Quartile 4	Full Sample
BM	0.563	0.532	0.535	0.578	0.552
	(0.513)	(0.443)	(0.431)	(0.505)	(0.473)
Size	6.433	7.112	7.275	7.066	6.971
	(7)	(8)	(8)	(7)	(7)
Own	6.735	6.870	7.040	6.787	6.858
	(7)	(7)	(8)	(7)	(7)
Analysts	9.648	12.157	12.793	12.114	11.678
	(7)	(10)	(11)	(11)	(11)
Debt	0.197	0.182	0.172	0.167	0.180
	(0.158)	(0.152)	(0.142)	(0.120)	(0.145)
Cash	0.286	0.312	0.275	0.210	0.277
	(0.185)	(0.215)	(0.180)	(0.137)	(0.180)
ROA	0.121	0.120	0.125	0.114	0.120
	(0.111)	(0.111)	(0.118)	(0.106)	(0.112)
ProgramSize	7.711	7.442	5.770	7.235	7.099
	(5.593)	(6.236)	(4.916)	(5.875)	(5.514)
Obs	2,528	2,528	2,528	2,528	10,112

Table 2.3. Probit Regression Results: Yearly Analysis of Net Income Performance

This table reports probit regression results examining whether firms announcing open market repurchase programs between 2004 and 2019 are more likely to exceed analyst net income forecasts in subsequent years. The dependent variable *Beat* equals one if quarterly net income exceeds the mean analyst forecast. The key independent variable *Announce* equals one for announcing firms and zero for matched firms. Results report average marginal effects (AMEs) with z-statistics in parentheses. Control variables, measured in the quarter before announcement, include: mutual fund ownership decile (*Own*), size decile (*Size*), analyst coverage (*analysts*, log of one plus number of analysts), book-to-market ratio (*BM*), leverage ratio (*Debt*, total debt/total assets), return on assets (*ROA*, operating income before depreciation/total assets), and cash flow ratio (*Cash*, operating cash flow/total debt). Event years 1-4 represent consecutive 12-month periods after the announcement, with year 1 excluding the first three months. All specifications include calendar-quarter fixed effects and standard errors are clustered at the firm level. *, **, and *** indicate significance at 10%, 5%, and 1%.

	Dependent Variable: $Beat$			
Variable	Year 1	Year 2	Year 3	Year 4
$Announce$	0.0252*	0.0337**	0.0394***	0.0278**
	(1.72)	(2.20)	(2.66)	(2.01)
Own	0.0037**	0.0031*	0.0036***	0.0046***
	(2.51)	(1.92)	(2.74)	(3.46)
$Size$	0.0070***	0.0047**	0.0090***	0.0079***
	(2.85)	(2.00)	(3.56)	(2.74)
$Analysts$	0.0084***	0.0350***	0.0136***	0.0074**
	(2.61)	(2.72)	(3.86)	(2.04)
BM	-0.0103**	-0.0106*	-0.0037*	-0.0109**
	(-2.05)	(-1.89)	(-1.69)	(-2.47)
$Debt$	-0.0172***	-0.0150**	-0.0198***	-0.0093*
	(-2.81)	(-2.33)	(-2.88)	(-1.69)
ROA	0.0332***	0.0276***	0.0290***	0.0393***
	(5.21)	(4.48)	(5.16)	(6.20)
$Cash$	-0.0447*	-0.0392	-0.0301	-0.0515**
	(-1.87)	(-1.64)	(-1.08)	(-2.26)
Observations	60,672	80,896	80,896	80,896

Table 2.4. Probit Regression Results: Yearly Analysis of Revenue Performance

This table reports probit regression results examining whether firms announcing open market repurchase programs between 2004 and 2019 are more likely to exceed analyst revenue forecasts in subsequent years. The dependent variable *beat* equals one if quarterly revenue exceeds the mean analyst forecast. The key independent variable *Announce* equals one for announcing firms and zero for matched firms. Results report average marginal effects (AMEs) with z-statistics in parentheses. Control variables, measured in the quarter before announcement, include: mutual fund ownership decile (*Own*), size decile (*Size*), analyst coverage (*Analysts*, log of one plus number of analysts), book-to-market ratio (*BM*), leverage ratio (*Debt*, total debt/total assets), return on assets (*ROA*, operating income before depreciation/total assets), and cash flow ratio (*Cash*, operating cash flow/total debt). Event years 1-4 represent consecutive 12-month periods after the announcement, with year 1 excluding the first three months. All specifications include calendar-quarter fixed effects and standard errors are clustered at the firm level. *, **, and *** indicate significance at 10%, 5%, and 1%.

	Dependent Variable: $Beat$			
Variable	Year 1	Year 2	Year 3	Year 4
$Announce$	0.0237	0.0291**	0.0338***	0.0280**
	(1.63)	(2.11)	(2.60)	(2.09)
Own	0.0027**	0.0032***	0.0031**	0.0022
	(2.02)	(2.74)	(2.47)	(1.47)
$Size$	0.0048**	0.0058***	0.0051**	0.0065***
	(2.14)	(2.73)	(2.17)	(3.15)
$Analysts$	0.0081**	0.0096***	0.0060	0.0084***
	(2.46)	(3.29)	(1.61)	(2.78)
BM	-0.0083*	-0.0062	-0.0052	-0.0038
	(-1.71)	(-1.20)	(-1.12)	(-0.84)
$Debt$	-0.0133**	-0.0157***	-0.0134**	-0.0154**
	(-2.29)	(-2.94)	(-2.54)	(-2.46)
ROA	0.0260***	0.0234***	0.0221***	0.0225***
	(5.21)	(5.14)	(4.45)	(5.17)
$Cash$	-0.0371	-0.0220	-0.0257	-0.0388*
	(-1.64)	(-1.09)	(-1.11)	(-1.66)
Observations	60,672	80,896	80,896	80,896

Table 2.5. Probit Regression Results by Completion Quartiles: Net Income Performance

This table examines how the likelihood of beating analyst net income forecasts varies with repurchase program completion rates in the four years following announcement. The sample consists of open market repurchase programs announced between 2004 and 2019. Firms are sorted into quartiles based on their program completion rate three months after announcement. For each completion quartile and event year, I estimate separate probit regressions where the dependent variable *Beat* equals one if quarterly net income exceeds the mean analyst forecast. The key independent variable *Announce* equals one for announcing firms and zero for matched firms. Results report average marginal effects (AMEs) with z-statistics in parentheses. All regressions include the same control variables as in Table 2.3 (mutual fund ownership decile, size decile, analyst coverage, book-to-market ratio, leverage ratio, return on assets, and cash flow ratio) but only the *Announce* AMEs are reported to save space. Event years 1-4 represent consecutive 12-month periods after the announcement, with year 1 excluding the first three months. All specifications include calendar-quarter fixed effects and standard errors are clustered at the firm level. *, **, and *** indicate significance at 10%, 5%, and 1% levels, respectively.

Variable	Year 1	Year 2	Year 3	Year 4
Quartile 1				
	0.0606***	0.0542***	0.0257	0.0035
	(2.96)	(2.68)	(1.25)	(0.18)
Quartile 2				
	0.0287	0.0278	0.0184	-0.0002
	(1.26)	(1.19)	(0.73)	(-0.09)
Quartile 3				
	0.0136	0.0247	0.0515**	0.0478*
	(0.76)	(1.42)	(2.03)	(1.75)
Quartile 4				
	0.0021	0.0256	0.0589***	0.0617***
	(0.13)	(1.39)	(2.95)	(3.13)

Table 2.6. Probit Regression Results by Completion Quartiles: Revenue Performance

This table examines how the likelihood of beating analyst revenue forecasts varies with repurchase program completion rates in the four years following announcement. The sample consists of open market repurchase programs announced between 2004 and 2019. Firms are sorted into quartiles based on their program completion rate three months after announcement. For each completion quartile and event year, I estimate separate probit regressions where the dependent variable *Beat* equals one if quarterly revenue exceeds the mean analyst forecast. The key independent variable *Announce* equals one for announcing firms and zero for matched firms. Results report average marginal effects (AMEs) with z-statistics in parentheses. All regressions include the same control variables as in Table 2.4 (mutual fund ownership decile, size decile, analyst coverage, book-to-market ratio, leverage ratio, return on assets, and cash flow ratio) but only the *Announce* AMEs are reported to save space. Event years 1-4 represent consecutive 12-month periods after the announcement, with year 1 excluding the first three months. All specifications include calendar-quarter fixed effects and standard errors are clustered at the firm level. *, **, and *** indicate significance at 10%, 5%, and 1% levels, respectively.

Variable	Year 1	Year 2	Year 3	Year 4
Quartile 1				
	0.0623***	0.0541***	0.0273	0.0096
	(3.18)	(2.78)	(1.27)	(0.38)
Quartile 2				
	0.0292*	0.0264	0.0137	0.0084
	(1.67)	(1.48)	(0.77)	(0.66)
Quartile 3				
	0.0114	0.0191	0.0347**	0.0309*
	(0.75)	(1.41)	(2.16)	(1.92)
Quartile 4				
	-0.0059	0.0201	0.0592***	0.0651***
	(-0.37)	(1.39)	(2.94)	(3.75)

Table 2.7. Probit Regression Results by Completion Quartiles: Net Income Performance Against Announcement-Time Estimates

This table examines how the likelihood of beating announcement-time analyst net income forecasts varies with repurchase program completion rates. The sample is restricted to programs announced within one month of the firm's fiscal year-end to ensure proper alignment between announcement timing and analyst forecasts. Firms are sorted into quartiles based on their program completion rate three months after announcement. For each completion quartile and event year, I estimate separate probit regressions where the dependent variable *Beat* equals one if fiscal year net income exceeds the mean analyst forecast made at the time of repurchase announcement. The key independent variable *Announce* equals one for announcing firms and zero for matched firms. Results report average marginal effects (AMEs) with z-statistics in parentheses. All regressions include the same control variables as in previous tables (mutual fund ownership decile, size decile, analyst coverage, book-to-market ratio, leverage ratio, return on assets, and cash flow ratio) but only the *Announce* AMEs are reported to save space. Event years 1-4 represent consecutive fiscal years after the announcement, with year 1 excluding the first three months. All specifications include calendar-quarter fixed effects and standard errors are clustered at the firm level. *, **, and *** indicate significance at 10%, 5%, and 1% levels, respectively.

Variable	Year 1	Year 2	Year 3	Year 4
Quartile 1				
	0.0757***	0.0871***	0.0675***	0.0574**
	(3.24)	(3.99)	(2.81)	(2.55)
Quartile 2				
	0.0356	0.0431*	0.0361	0.0211
	(1.31)	(1.86)	(1.18)	(0.72)
Quartile 3				
	0.0126	0.0281	0.0496***	0.0687***
	(0.57)	(1.02)	(2.58)	(3.28)
Quartile 4				
	0.0081	0.0418*	0.0932***	0.1198***
	(0.31)	(1.65)	(3.87)	(4.12)

Table 2.8. Probit Regression Results by Completion Quartiles: Revenue Performance Against Announcement-Time Estimates

This table examines how the likelihood of beating announcement-time analyst revenue forecasts varies with repurchase program completion rates. The sample is restricted to programs announced within one month of the firm's fiscal year-end to ensure proper alignment between announcement timing and analyst forecasts. Firms are sorted into quartiles based on their program completion rate three months after announcement. For each completion quartile and event year, I estimate separate probit regressions where the dependent variable *Beat* equals one if fiscal year revenue exceeds the mean analyst forecast made at the time of repurchase announcement. The key independent variable *Announce* equals one for announcing firms and zero for matched firms. Results report average marginal effects (AMEs) with z-statistics in parentheses. All regressions include the same control variables as in previous tables (mutual fund ownership decile, size decile, analyst coverage, book-to-market ratio, leverage ratio, return on assets, and cash flow ratio) but only the *Announce* AMEs are reported to save space. Event years 1-4 represent consecutive fiscal years after the announcement, with year 1 excluding the first three months. All specifications include calendar-quarter fixed effects and standard errors are clustered at the firm level. *, **, and *** indicate significance at 10%, 5%, and 1% levels, respectively.

Variable	Year 1	Year 2	Year 3	Year 4
Quartile 1				
	0.0651**	0.0914***	0.0729***	0.0677**
	(2.54)	(3.54)	(2.83)	(2.52)
Quartile 2				
	0.0401	0.0514**	0.0423*	0.0392
	(1.57)	(2.00)	(1.71)	(1.57)
Quartile 3				
	0.0208	0.0291*	0.0588**	0.0712***
	(1.21)	(1.70)	(2.47)	(2.89)
Quartile 4				
	0.0127	0.0407	0.0825***	0.1126***
	(0.67)	(1.32)	(2.64)	(3.23)

Table 2.9. Abnormal Returns Around Earnings Announcements by Completion Quartiles

This table reports mean and median cumulative abnormal returns (CARs) around quarterly earnings announcements for open market repurchase programs announced between 2004 and 2019. Firms are sorted into quartiles based on their program completion rate three months after announcement. CARs are computed over a four-day window [−1,+2] around each earnings announcement using a one-factor market model with the equal-weighted market index estimated over trading days [−250,−10]. Event years 1-4 represent consecutive 12-month periods after the announcement, with year 1 excluding the first three months. *Panel A* reports mean CARs estimated as intercepts from constant-only regressions with standard errors clustered by earnings announcement date. *Panel B* reports median CARs, where monthly medians within each quartile-year group are tested using Newey-West HAC regressions (lag 5). P-values are shown in parentheses. All specifications account for same-date clustering of earnings announcements. *, **, and *** indicate significance at 10%, 5%, and 1% levels, respectively.

	Year 1	Year 2	Year 3	Year 4
Panel A: Mean				
Quartile 1	0.6578***	0.5928***	0.2273	-0.2268
	(0.003)	(0.007)	(0.362)	(0.310)
Quartile 2	0.3867*	0.2371	0.0645	0.0192
	(0.091)	(0.242)	(0.762)	(0.935)
Quartile 3	0.1981	0.2847	0.6279***	0.3656*
	(0.315)	(0.210)	(0.005)	(0.092)
Quartile 4	0.1560	0.3223	0.4082***	0.3232***
	(0.385)	(0.149)	(0.000)	(0.001)
Panel B: Median				
Quartile 1	0.3049***	0.3187***	0.1403	0.0462
	(0.001)	(0.000)	(0.139)	(0.589)
Quartile 2	0.1910**	0.1506*	0.0687	0.0386
	(0.033)	(0.096)	(0.397)	(0.654)
Quartile 3	0.1251	0.1569	0.2849***	0.2009**
	(0.217)	(0.123)	(0.005)	(0.018)
Quartile 4	0.0895	0.1423	0.7347***	0.6251***
	(0.336)	(0.115)	(0.000)	(0.000)

Table 2.10. Long-Run Abnormal Returns by Completion Quartiles

This table reports daily abnormal returns (alphas) from Carhart four-factor regressions for open market repurchase programs announced between 2004 and 2019. Firms are sorted into quartiles based on their program completion rate three months after announcement. For each firm, I estimate separate four-factor regressions for each of the first four event years following the announcement, as well as over the entire four-year period. Event years 1-4 represent consecutive 12-month periods after the announcement, with year 1 excluding the first three months (months 4-12, 13-24, 25-36, and 37-48, respectively). The 4-year column covers the full period (months 4-48). *Panel A* reports mean alphas with p-values from t-tests in parentheses. *Panel B* reports median alphas with p-values from Wilcoxon signed-rank tests in parentheses. Alphas are reported in percentage points per day. *, **, and *** indicate significance at 10%, 5%, and 1% levels, respectively.

	Year 1	Year 2	Year 3	Year 4	4-Year
Panel A: Mean					
Quartile 1	0.0299***	0.0277***	0.0085	-0.0023	0.0155***
	(0.000)	(0.000)	(0.251)	(0.212)	(0.000)
Quartile 2	0.0198***	0.0174**	0.0053	-0.0017	0.0104***
	(0.008)	(0.017)	(0.686)	(0.690)	(0.001)
Quartile 3	0.0074	0.0123*	0.0211**	0.0174**	0.0140***
	(0.114)	(0.074)	(0.011)	(0.042)	(0.000)
Quartile 4	0.0135**	0.0142*	0.0275***	0.0253***	0.0192***
	(0.026)	(0.068)	(0.000)	(0.000)	(0.000)
Panel B: Median					
Quartile 1	0.0109***	0.0113***	0.0031	0.0019	0.0072***
	(0.000)	(0.000)	(0.146)	(0.315)	(0.000)
Quartile 2	0.0070***	0.0067**	0.0018	-0.0006	0.0048***
	(0.005)	(0.017)	(0.221)	(0.722)	(0.000)
Quartile 3	0.0027	0.0043**	0.0081***	0.0058***	0.0065***
	(0.102)	(0.047)	(0.000)	(0.009)	(0.000)
Quartile 4	0.0053**	0.0051**	0.0098***	0.0078***	0.0092***
	(0.032)	(0.046)	(0.000)	(0.000)	(0.000)

Table 2.11. Calendar-Time Alphas by Completion Quartiles and Event Year

This table reports monthly calendar-time alphas from Carhart four-factor regressions for open market repurchase programs announced between 2004 and 2019. Firms are sorted into quartiles based on their program completion rate three months after announcement. For each quartile-event year group, I form equal-weighted portfolios each calendar month from all firms belonging to that group, creating 16 return series (4 quartiles × 4 years). Each series is regressed on market, size, value, and momentum factors with the intercept reported as alpha. Event years 1-4 represent consecutive 12-month periods after the announcement, with year 1 excluding the first three months (months 4-12, 13-24, 25-36, and 37-48, respectively). The 4-year column pools months 4-48 by quartile across all event years. Alphas are estimated using OLS with Newey-West HAC standard errors (lag 6) and reported in percentage points per month. P-values are in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1% levels, respectively.

	Year 1	Year 2	Year 3	Year 4	4-Year
Quartile 1	0.5257***	0.5410***	0.2085	-0.0756	0.3629***
	(0.000)	(0.000)	(0.129)	(0.528)	(0.003)
Quartile 2	0.3677***	0.3067**	0.1330	0.0104	0.2356**
	(0.009)	(0.018)	(0.281)	(0.829)	(0.011)
Quartile 3	0.1740*	0.2937*	0.4328**	0.4022***	0.2862***
	(0.065)	(0.053)	(0.011)	(0.008)	(0.008)
Quartile 4	0.3036*	0.3412**	0.5437***	0.5878***	0.4049***
	(0.070)	(0.029)	(0.002)	(0.000)	(0.000)

Table 2.12. Performance Comparison: Completion-Based vs. Traditional Strategies

This table compares the performance of two investment strategies based on open market repurchase announcements from 2004 to 2019. The *Classic* strategy holds an equal-weighted portfolio of all firms with authorizations in months 7-48 after announcement. The *Refined* strategy conditions on completion rates, holding low-completion firms (quartile 1) in months 7-24 and high-completion firms (quartile 4) in months 31-48. Both strategies use equal-weighted portfolios. Alphas are estimated from Carhart four-factor regressions with Newey-West HAC standard errors (lag 6). Turnover represents average one-way monthly portfolio turnover. *Panel A* reports gross alphas. *Panel B* reports net alphas after subtracting 10 basis points one-way trading costs. Alphas are in percentage points per month with p-values in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1% levels, respectively.

	Refined	Classic	Refined − Classic
Panel A: Gross			
Alpha (%/month)	0.5529***	0.2992***	0.2536**
	(0.000)	(0.006)	(0.036)
Turnover (%/month)	8.75	4.77	–
Panel B: Net (10 bps costs)			
Alpha (%/month)	0.5441***	0.2944***	0.2497**
	(0.000)	(0.006)	(0.038)

Chapter 3

Open Market Repurchase Programs and Systematic Liquidity

3.1 Introduction

Open market share repurchase programs (OMRs) have become the dominant form of corporate payout over the past two decades, surpassing dividends in both the United States and Europe (Anolick et al., 2021). While early research primarily focused on understanding the motivations and valuation effects of OMR announcements, a growing body of literature has shifted attention toward the consequences of firms actively trading their own shares in the market.

A central topic in this line of research is the effect of OMRs on firms' liquidity. Early work by Barclay and Smith (1988) highlighted the adverse selection costs introduced when informed firms trade their own shares, finding that bid-ask spreads tend to widen during repurchase programs. However, other studies argue that the long duration and flexibility of OMRs allow firms to act as patient liquidity providers, improving market liquidity (Wiggins, 1994; Franz et al., 1995; Nayar et al., 2008; Hillert et al., 2016). Relatedly, Hong et al. (2008) modeled repurchasing firms as buyers of last resort, showing that such intervention reduces short-term return variance and idiosyncratic risk. Similar to this notion, Busch and Obernberger (2017) show that the stabilizing effect of firms during open market repurchase programs makes prices more efficient and reduces idiosyncratic risk.

While prior studies have focused primarily on firm-level liquidity effects, the implications of open market repurchase programs (OMRs) for systematic liquidity remain largely unexplored. Systematic liquidity, or liquidity commonality, refers to the shared component of liquidity variation across assets. A growing body of empirical evidence documents the presence of liquidity commonality across a wide range of asset classes—including equities, bonds, and derivatives—and across both U.S. and international markets (Chordia et al., 2000; Hasbrouck and Seppi, 2001; Huberman and Halka, 2001; Karolyi et al., 2012).

Importantly, systematic liquidity is not just a microstructure curiosity; it has critical implications for asset pricing. Studies show that a stock's exposure to systematic liquidity risk—that is, whether its liquidity dries up at inopportune times—matters for investors and commands a risk premium (see Pástor and Stambaugh, 2003; Acharya and Pedersen, 2005; Sadka, 2006; Korajczyk and Sadka, 2008). For example, Acharya and Pedersen (2005) develop an asset pricing model where stocks that maintain liquidity during market downturns earn lower average returns, as investors value the option to exit at reasonable cost when market-wide liquidity evaporates.

What explains the existence of systematic liquidity? The literature points to both supply-side and demand-side mechanisms. On the supply side, theoretical models such as Brunnermeier and Pedersen (2009) show that large market declines or spikes in volatility impair the funding liquidity of financial intermediaries (e.g., market makers) who provide liquidity across multiple securities. As these intermediaries face tighter constraints, they reduce liquidity provision broadly, generating co-movement in asset liquidity. Empirically, Coughenour and Saad (2004) find that stocks handled by the same NYSE specialist exhibit stronger liquidity commonality, while Hameed et al. (2010) show that liquidity co-movement intensifies following negative market returns or during periods of high volatility.

On the demand side, liquidity commonality can arise from correlated trading behavior across institutional investors. When institutional investors face liquidity shocks or shifts in risk appetite, their common ownership and synchronized trading across portfolios generate simultaneous liquidity pressures across multiple assets. For example, when mutual funds experience redemptions, they may sell a broad swath of holdings at the same time, producing widespread liquidity stress. Koch et al. (2016) show that stocks with high mutual fund ownership exhibit about twice the liquidity comovement of those with low mutual fund ownership, and Kamara et al. (2008) document that changes in the cross-sectional distribution of liquidity commonality over 1963–2005 can be explained by evolving institutional ownership patterns.

This paper argues that firms engaging in open market repurchase programs (OMRs) occupy a unique position in the market—one that insulates them from, and allows them to counteract, the shocks that typically drive systematic liquidity. Unlike market makers and institutional investors, repurchasing firms are informed traders who use internal cash to buy back a single security—their own shares. As a result, they are largely unaffected by the funding constraints or redemption pressures that amplify supply- or demand-side liquidity shocks. Crucially, these firms not only possess the ability but also the incentive to trade against such shocks, acting as buyers of last resort when liquidity dries up in the broader market. For example, during periods of mutual fund outflows, when systematic selling pressure leads to widespread increases in bid-ask spreads, repurchasing firms can step in to absorb order flow and stabilize liquidity in their own stock. This reasoning leads to a clear empirical prediction: during active OMR programs, firms should exhibit a lower degree of liquidity commonality with the market.

To test this hypothesis, I examine a sample of 1,095 open market repurchase programs announced between 1993 and 2019. For each repurchasing firm, I estimate liquidity commonality using three different liquidity measures: the dollar quoted bid-ask spread, the percentage quoted bid-ask spread, and the Amihud illiquidity ratio. I compute commonality coefficients over three distinct event windows: (1) a pre-repurchase window, defined as the six months preceding the OMR announcement; (2) a repurchase window, covering the six months following the announcement; and (3) a post-repurchase window, which begins one year after the announcement and extends for six months. Comparing firms' liquidity commonality across these windows reveals a striking pattern. From the pre-repurchase period (window 1) to the repurchase period (window 2), liquidity commonality declines sharply and significantly across all three liquidity measures. This reduction is both statistically and economically meaningful; for example, when measured using the percentage quoted bid-ask spread, firms' liquidity commonality falls by an average of 14.9% relative to pre-repurchase levels. Notably, this decline proves to be temporary: in the post-repurchase window (window 3), liquidity commonality rebounds and returns to levels comparable to those observed before the OMR announcement.

To ensure that my findings are not driven by time- or industry-specific factors, I construct a matched sample of non-repurchasing firms. For each firm engaging in an OMR, I identify a matching firm operating in the same industry, belonging to the same size decile, and with a similar book-to-market ratio. I then calculate the adjusted change in liquidity commonality by subtracting the change observed in the matching firm from the change observed in the repurchasing firm (for example, $\Delta\beta_i^{adj} = (\beta_{i,2} - \beta_{i,1}) - (\beta_{match,2} - \beta_{match,1})$. The results confirm the same striking pattern: a significant and economically meaningful decline in liquidity commonality between the pre-repurchase and repurchase windows, followed by a rebound to pre-repurchase levels in the post-repurchase window.

Importantly, I also test whether this reduction in liquidity commonality is symmetric across market conditions. Because OMRs only allow firms to buy their own shares, firms can neutralize order imbalances only when there is selling pressure; they cannot intervene when there is excess buying. To account for this asymmetry, I split liquidity commonality into two components—β_i^n, estimated on days when market-wide order imbalance is negative (net selling pressure), and β_i^p, estimated on days with positive order imbalance (net buying pressure). I instrument firm-level order imbalance using the cross-sectional equal-weighted market order imbalance, following Hasbrouck and Seppi (2001), to avoid endogeneity from firms' own repurchase activity. The results reveal that the decline in liquidity commonality between window 1 and window 2, and its subsequent rebound in window 3, is concentrated entirely in β_i^n (negative imbalance days). In contrast, β_i^p remains stable across all windows. This asymmetry strongly supports the interpretation that firms act as buyers of last resort, buffering liquidity shocks specifically during periods of market-wide selling pressure.

I further investigate whether repurchasing firms primarily neutralize demand-side or supply-side liquidity shocks—and find evidence for both. On the demand side, I examine the relationship between institutional ownership and liquidity commonality, estimating separate liquidity betas for negative and positive market order imbalance days. Consistent with prior work (e.g., Kamara et al. (2008)), I find that before and after the repurchase window, firms with higher institutional ownership exhibit stronger liquidity comovement, reflecting the influence of common ownership and correlated trading. However, this relationship disappears during the repurchase window for negative order imbalance days: institutional ownership no longer explains variation in liquidity commonality when firms are actively repurchasing shares. This suggests that repurchasing firms neutralize the institutional channel of systematic liquidity shocks.

On the supply side, I follow the literature in linking market makers' liquidity provision constraints to market volatility and short-term borrowing costs (proxied by the TED spread). I find that pre- and post-repurchase, shocks to volatility and funding costs significantly amplify liquidity commonality, but during repurchase windows, these effects vanish: volatility and TED spread shocks no longer explain variation in firms' liquidity comovement. Together, these results indicate that repurchasing firms buffer their stocks against both demand- and supply-side drivers of systematic liquidity shocks.

Finally, I examine whether the reduction in liquidity commonality during OMR programs translates into a change in firms' liquidity risk. Building on the liquidity-adjusted asset pricing framework of Pástor and Stambaugh (2003) and Acharya and Pedersen (2005), I estimate firms' exposures (betas) to a traded liquidity risk factor, alongside standard market, size, value, and momentum factors. Using a replicated daily liquidity factor portfolio, I find that firms experience a significant decline in liquidity risk during repurchase periods, with no comparable changes in other factor loadings. On average, firms' liquidity betas decrease by approximately 0.06, corresponding to a 1.2% annualized reduction in their cost of capital—a meaningful and economically significant effect. Notably, this reduction proves temporary: once OMR programs conclude, liquidity betas revert to pre-repurchase levels. Correlation analysis confirms that reductions in liquidity commonality and liquidity risk move closely together, supporting the interpretation that OMR programs provide firms with temporary insulation from systematic liquidity risk.

3.2 Data

I obtain data on share repurchase programs from the SDC Mergers and Acquisitions database, one of the most widely used public sources for tracking repurchase activity, covering U.S. firms since 1984. To construct the sample for this study, I apply several filters designed to ensure clean measurement of firms' liquidity commonality and liquidity risk around open market repurchase (OMR) programs. Specifically, a repurchase program is included if it satisfies the following conditions:

1. The program is labeled as an open market repurchase (OMR), excluding other types such as tender offers, Dutch auctions, odd-lot repurchases, and accelerated repurchase programs.
2. The program is marked as completed in SDC.
3. The security repurchased is a common stock traded on NYSE, NASDAQ, or AMEX.
4. Information on program size (as a percentage of shares outstanding) and completion rate (the fraction of announced shares actually repurchased) is available.
5. The firm has no active repurchase program during the six months prior to the OMR announcement (pre-announcement window) and does not announce a new repurchase program within 18 months following the announcement (ensuring a clean post-repurchase window).
6. Trading and price data are available in TAQ and CRSP over the period spanning six months prior to announcement through 18 months after.

Although these filters reduce the sample size, they are necessary to isolate the effects of firms' trading activity during OMRs. For example, limiting the sample to open market programs ensures that the analysis focuses on discretionary, flexible repurchases, as opposed to one-off or structured transactions. Similarly, requiring the absence of overlapping programs helps ensure that the pre- and post-repurchase windows are free of contamination from other buyback activity. While I limit the sample to programs marked as completed, I note that my main results—particularly the temporary nature of the liquidity commonality reduction—are difficult to explain solely by completion-related selection effects.

After applying these filters, the final sample consists of 1,095 OMR programs announced between April 9, 1993, and October 8, 2019. To provide a sense of firm and program characteristics, Table 3.1 reports summary statistics. Firms undertaking OMRs tend to be large, with mean (median) market capitalization deciles of 7.7 (7), and have relatively high book-to-market ratios, with a mean (median) of 1.9 (1.5), similar to findings in Grullon and Michaely (2004).

Repurchase programs are economically meaningful in size and often span extended periods. The mean (median) program duration is 404 (283) days, with nearly 7% of programs lasting more than two years. Program size, defined as the percentage of shares outstanding announced for repurchase, averages 7.7% (median 5.0%).

Trading data are drawn from the TAQ tools provided via Wharton Research Data Services, covering daily trade characteristics since January 1993. I construct three primary liquidity measures: (1) dollar quoted bid-ask spread (average daily spread in dollars), (2) percentage quoted spread (average daily spread divided by midpoint), and (3) Amihud illiquidity measure, calculated as the ratio of absolute daily return to daily dollar trading volume Amihud (2002). Additionally, I compute daily order imbalance as the dollar value of buy trades minus the dollar value of sell trades for each firm.

3.3 OMR and Liquidity Commonality

3.3.1 Baseline and Adjusted Analysis

To estimate changes in liquidity commonality following the initiation of open market repurchase (OMR) programs, I use a market model similar to that of Chordia et al. (2000). Specifically, in equation (3.1), $DL_{i,d}$ denotes the percentage change in the liquidity of stock i from trading day $d-1$ to day d, while $DL_{M,d}$ represents the corresponding change in the cross-sectional average liquidity of the market (excluding stock i). The coefficient β_i captures the degree of comovement between the liquidity of stock i and market liquidity. To control for potential confounding effects, I include one lead and one lag of market liquidity, contemporaneous, lead, and lagged market returns, as well as the contemporaneous change in the squared return of the individual stock. The leads and lags account for delayed adjustments in liquidity commonality, the market return controls for spurious dependence between returns and liquidity, and the squared return proxies for stock-level volatility, which may itself influence liquidity.

$$DL_{i,d} = \alpha_i + \beta_i DL_{M,d} + \text{controls} + \epsilon_{i,d} \tag{3.1}$$

For each stock, the market liquidity measure $DL_{M,d}$ is calculated excluding that stock to avoid mechanical correlation. To ensure robustness, I estimate equation (3.1) separately using three liquidity measures: dollar quoted bid-ask spread ($QSPR$), percentage quoted bid-ask spread ($PQSPR$), and the Amihud illiquidity measure ($Amihud$).

To capture temporal variation, I define three estimation windows: (1) **Window 1 (pre-repurchase)** — the six months prior to the OMR announcement, (2) **Window 2 (repurchase period)** — the six months following the announcement, and (3) **Window 3 (post-repurchase)** — a six-month window beginning one year after the announcement.

These windows are chosen to ensure sufficient daily observations for reliable coefficient estimates, to align with the typical program duration (median of nine months), and to avoid contamination from overlapping programs (as ensured by the sample filters described in Section 3.2).

I estimate equation (3.1) separately for each firm and window, and capture changes in liquidity commonality by examining shifts in the estimated β_i coefficients across periods. Panel A of Table 3.2 reports the change from Window 1 to Window 2 ($\Delta\beta_i = \beta_i^{\text{window 2}} - \beta_i^{\text{window 1}}$). The results show economically and statistically significant reductions in liquidity commonality across all three measures. Specifically, the mean (median) reductions are -0.176 (-0.092) for $QSPR$, -0.145 (-0.076) for $PQSPR$, and -0.163 (-0.086) for $Amihud$. When normalized by the average pre-repurchase levels, these reductions represent declines of approximately -14.6% (-11.3%), -14.9% (-10.7%), and -15.2% (-11.4%) respectively, underscoring their economic significance.

Panel B of Table 3.2 examines the change from Window 2 to Window 3 ($\Delta\beta_i = \beta_i^{\text{window 3}} - \beta_i^{\text{window 2}}$). If the observed reduction during Window 2 reflects the stabilizing effects of my repurchasing activity, I would expect liquidity commonality to rebound after the program concludes. Consistent with this prediction, the results show significant increases across all three measures, with mean (median) changes of 0.181 (0.095) for $QSPR$, 0.150 (0.081) for $PQSPR$, and 0.159 (0.084) for $Amihud$.

A comparison of Panels A and B shows that the magnitude of the post-repurchase rebound closely mirrors the earlier decline, and the commonality levels in Window 3 are statistically indistinguishable from those in Window 1. This pattern supports the interpretation that repurchasing firms temporarily act as buyers of last resort, buffering their stocks from systematic liquidity shocks during the program but reverting to market-level comovement once the program ends. Overall, the results in Table 3.2 provide strong evidence consistent with this stabilizing mechanism.

To address potential concerns about firm-, industry-, or time-specific effects driving the observed patterns, I perform a matched-firm adjustment. Specifically, for each repurchasing firm, I identify a non-repurchasing firm that serves as a control. The matched firm is required to (1) belong to the same industry, (2) fall within the same size decile, and (3) not be engaged in any open market repurchase program during the estimation windows. If multiple firms meet these criteria, I select the one with the closest book-to-market ratio.

For each firm-program observation, I then adjust the change in liquidity commonality by subtracting the corresponding change observed in its matched non-repurchasing firm.

Formally, the adjusted change is defined as:

$$\Delta\beta_i^{\text{adjusted}} = \Delta\beta_i - \Delta\beta_{\text{matched}}, \tag{3.2}$$

where $\Delta\beta_i$ is the change in liquidity commonality coefficient for the repurchasing firm between two windows (e.g., Window 2 minus Window 1), and $\Delta\beta_{\text{matched}}$ is the analogous change for the matched firm.

Table 3.3 presents the adjusted changes in liquidity commonality. The results mirror those observed in Table 3.2: there is a significant decline in commonality from Window 1 to Window 2, followed by an offsetting increase from Window 2 to Window 3, such that the levels in Window 1 and Window 3 are statistically indistinguishable. The similarity of the raw and adjusted results suggests that the patterns I observe are unlikely to be artifacts of firm-specific characteristics, industry trends, or broad market conditions.

3.3.2 Selling vs. Buying Markets

As discussed earlier, when firms act as buyers of last resort during repurchase programs, they have the potential to counteract both demand- and supply-side liquidity shocks, thereby reducing the comovement between their own liquidity and aggregate market liquidity. The results in Tables 3.2 and 3.3 are consistent with this mechanism. However, it is important to note that repurchase programs provide firms with an asymmetric intervention capacity: they allow firms to increase buying activity when facing selling pressure, but they do not permit selling when markets experience excess buying.

This asymmetry leads to a clear empirical prediction: the reduction in liquidity commonality during repurchase programs should be concentrated on days characterized by net selling pressure, with little or no effect observed on days with net buying pressure.

To test this prediction, I use market-wide order imbalance as a proxy for aggregate buying and selling pressure. Specifically, I compute daily order imbalance as the dollar value of buy trades minus the dollar value of sell trades, averaged across all NYSE stocks (equal-weighted), using TAQ data from the Wharton Research Data Services. Because firm-level order imbalance for repurchasing firms is mechanically influenced by their own buyback activity, I rely on market-level order imbalance as an instrument, following Hasbrouck and Seppi (2001), which documents strong cross-firm commonality in order imbalances.

To formally test the asymmetry, I extend equation (3.1) by allowing for separate liquidity commonality coefficients on days with positive versus negative market order imbalance. Specifically, I estimate the following specification:

$$DL_{i,d} = \alpha_i + \beta_i^p DL_{M,d} \cdot \mathbb{I}[IMB_{M,d} \geq 0] + \beta_i^n DL_{M,d} \cdot \mathbb{I}[IMB_{M,d} < 0] + \text{controls} + \epsilon_{i,d} \tag{3.3}$$

where $DL_{i,d}$ is the percentage change in liquidity for firm i on day d, $DL_{M,d}$ is the percentage change in market liquidity (excluding firm i), and $IMB_{M,d}$ is the market-wide order imbalance. The coefficients β_i^p and β_i^n capture liquidity commonality on days of positive and negative market order imbalance, respectively.

I estimate this model for each firm-program observation over the three event windows, focusing on the $QSPR$ liquidity measure for brevity. Panel A of Table 3.4 reports the change in commonality coefficients from Window 1 to Window 2. Consistent with the prediction, the results show a significant reduction in β_i^n (negative imbalance days), with a mean (median) change of -0.382 (-0.215), while β_i^p (positive imbalance days) shows no significant change over the same period.

Panel B of Table 3.4 reports the change from Window 2 to Window 3. As with the overall commonality results, I expect the reduction in β_i^n to reverse once the repurchase program concludes. Indeed, the results show a significant increase in β_i^n after the program ends, with a magnitude comparable to the earlier decline. In contrast, β_i^p remains stable across all windows.

Overall, these findings reinforce the interpretation that OMR programs reduce liquidity commonality specifically by mitigating the effects of systematic selling pressure, consistent with firms' asymmetric role as buyers—but not sellers—of last resort. These results complement the findings of Hillert et al. (2016), who demonstrate that OMR programs improve individual firm liquidity levels. Together, these findings provide converging evidence that repurchasing firms act as comprehensive liquidity providers: they not only enhance their own stock's liquidity but also reduce their dependence on market-wide liquidity conditions by selectively counteracting systematic selling pressure. This dual role—improving both liquidity levels and reducing liquidity comovement—underscores the stabilizing market-making function that firms perform during repurchase programs.

3.4 Stabilizing Effect Mechanisms

Liquidity commonality is one of the most robust empirical patterns in the market microstructure literature, and prior research has identified both demand-side and supply-side forces as key contributors. On the demand side, institutional investors with overlapping holdings and correlated trading behaviors generate synchronized liquidity shocks across assets. On the supply side, constraints faced by market makers—such as funding or inventory risk—create cross-asset variation in liquidity provision. Evidence supporting both mechanisms has been documented across multiple markets and time periods, suggesting that both play meaningful roles in driving liquidity comovement.

Having documented that repurchasing firms experience a significant and temporary reduction in liquidity commonality during OMR programs, the next question is: against which sources of systematic liquidity shocks do firms provide a buffer? Specifically, do firms primarily neutralize the demand-side effects associated with institutional trading, the supply-side effects linked to market maker constraints, or both? This section investigates these questions.

3.4.1 Demand-Side Mechanism

If institutional ownership and correlated institutional trading are important drivers of liquidity commonality, then firms with higher institutional ownership should exhibit greater liquidity comovement on average. Prior studies support this view: for example, Koch et al. (2016) show that liquidity comovement is roughly twice as high among stocks with heavy mutual fund ownership, while Kamara et al. (2008) link cross-sectional variation in liquidity commonality to institutional holdings.

To assess whether repurchasing firms counteract institutional trading effects, I examine the relationship between institutional ownership and liquidity commonality across the event windows. Institutional ownership data are obtained from the CDA/Spectrum database, measured as the percentage of shares held by institutions in the quarter prior to the OMR announcement (denoted $inst$). For each firm and window, I regress the estimated liquidity commonality coefficients (β_i^p for positive market order imbalance days and β_i^n for negative imbalance days) on $inst$.

Table 3.5 reports the results. For β_i^p, there is a consistently positive and significant relationship with institutional ownership across all three windows, indicating that higher institutional holdings are associated with greater liquidity comovement on buying-pressure days. For β_i^n, however, the pattern is notably different: institutional ownership is positively related to liquidity commonality before and after the repurchase window, but this relationship disappears during the repurchase period itself. The explanatory power of institutional ownership (as measured by R^2) similarly collapses for β_i^n during the repurchase window, falling from 8% and 7% in the pre- and post-repurchase periods to nearly zero.

These results suggest that firms' repurchasing activity effectively neutralizes the institutional demand-side channel of liquidity commonality, specifically on days with net selling pressure.

3.4.2 Supply-Side Mechanism

To examine whether firms also buffer supply-side liquidity shocks, I focus on two variables commonly associated with market makers' ability to provide liquidity: market volatility and short-term funding costs. Elevated volatility increases inventory risk, while higher short-term rates tighten funding constraints; both mechanisms can amplify cross-asset liquidity comovement Chordia et al. (2000); Brunnermeier and Pedersen (2009); Kamara et al. (2008).

Daily market volatility shocks are estimated following Schwert (1990): I first regress daily market returns on an intercept, weekly dummies, and 22 lags; the absolute residuals from this regression are then regressed on an intercept and 22 lags, with the residuals representing daily volatility shocks ($\sigma_{M,d}$). To capture funding shocks, I use the TED spread—the difference between the three-month Treasury bill rate and three-month LIBOR—where daily shocks (TED_d) are obtained by regressing the TED spread on its 22 lags and extracting the residual.

I then estimate the following model:

$$DL_{i,d} = \alpha_i + \beta_i DL_{M,d} + t_i TED_d \cdot DL_{M,d} + s_i \sigma_{M,d} \cdot DL_{M,d} + \mu_{i,d} \tag{3.4}$$

where $DL_{i,d}$ is the percentage change in firm-level liquidity, and $DL_{M,d}$ is the cross-sectional average change (excluding firm i). Here, t_i and s_i capture the excess sensitivity of firm i's liquidity commonality to TED spread and volatility shocks, respectively.

Table 3.6 presents the results. Both t_i and s_i are positive and significant during the pre- and post-repurchase windows, indicating that, consistent with prior literature, higher funding costs and volatility increase liquidity comovement. Strikingly, however, these relationships vanish during the repurchase window: the average and median coefficients become statistically insignificant, and in some cases even flip sign.

Together, these findings indicate that firms not only dampen demand-side liquidity shocks during OMR programs, but also buffer the effects of supply-side shocks related to market maker constraints. This dual stabilizing role highlights the unique position of repurchasing firms as liquidity providers in the market.

3.5 Liquidity Risk

An extensive literature has explored liquidity as a potential risk factor, emphasizing that liquidity varies over time and that its variation has a systematic component Pástor and Stambaugh (2003); Sadka (2006); Acharya and Pedersen (2005). In particular, Acharya and Pedersen (2005) propose a liquidity-adjusted capital asset pricing model in which expected returns depend not only on market beta but also on several forms of liquidity risk, including the comovement between firm-level and market-level liquidity. This highlights the close conceptual connection between liquidity commonality and liquidity risk.

Given the evidence documented in earlier sections—namely, that liquidity commonality declines significantly during repurchase programs—one would expect a corresponding change in firms' exposure to systematic liquidity risk. This section investigates that connection.

To estimate liquidity risk, I employ a five-factor asset pricing model similar to Pástor and Stambaugh (2003), which allows for the inclusion of a tradable liquidity risk factor alongside the standard Fama-French-Carhart factors (market, size, value, momentum). This model is well suited to my setting because it can be estimated at daily frequency, matching the design of the six-month event windows. Since no publicly available daily series of the Pastor-Stambaugh liquidity factor exists, I replicate the tradable liquidity portfolio following procedures in Pástor and Stambaugh (2003), Li et al. (2019), and Pontiff and Singla (2019). To validate the replication, I compare the monthly returns of my constructed factor to those published by Lucas Stambaugh and the Wharton Research Data Services, finding correlations exceeding 99%.

For each firm-program observation, I estimate the following regression separately over Window 1 (pre-repurchase), Window 2 (repurchase period), and Window 3 (post-repurchase):

$$r_{i,d} = \beta_i^{mkt} MKT_d + \beta_i^{smb} SMB_d + \beta_i^{hml} HML_d + \beta_i^{mom} MOM_d + \beta_i^{ps} PS_d + \epsilon_{i,d} \tag{3.5}$$

where $r_{i,d}$ is the daily return of firm i, and PS_d is the daily return on the replicated liquidity factor.

Panel A of Table 3.7 reports the changes in factor loadings from Window 1 to Window 2. While market, value, and momentum betas remain stable, both size and liquidity betas exhibit significant reductions. Notably, the mean (median) reduction in the liquidity beta is -0.06 (-0.07), with p-values below 0.01, indicating a meaningful decline in firms' exposure to systematic liquidity risk during repurchase periods. To gauge the economic significance, I multiply the change in liquidity beta by the average return on the liquidity factor, finding that the reduction in liquidity risk translates into an average annual cost of capital decrease of approximately 1.2% (median 0.88%), significant at the 1% level.

These findings provide an interesting contrast to Grullon and Michaely (2004), who document significant reductions in market beta following share repurchase announcements using a three-factor model. While I observe directionally similar changes in market beta during repurchase periods, these changes are economically small and statistically insignificant in my specification. Instead, the primary drivers of cost of capital reduction appear to be the decline in size factor loading and, most notably, the substantial reduction in liquidity risk exposure. This suggests that the inclusion of liquidity as an explicit risk factor may provide a more complete characterization of the risk changes associated with repurchase programs. Moreover, unlike prior studies that focus primarily on the immediate post-announcement period, my analysis of the full program lifecycle reveals that any cost of capital benefits are temporary, with factor loadings reverting to pre-announcement levels after program completion.

Panel B of Table 3.7 shows that these changes are temporary: liquidity betas rebound from Window 2 to Window 3, with increases of similar magnitude to the earlier declines, while other factor loadings remain unchanged. This suggests that the liquidity risk reduction coincides specifically with the repurchase window.

To ensure these patterns are not driven by industry-time fixed effects, I apply the matched-firm adjustment described previously. Table 3.8 presents the adjusted results, which closely mirror the unadjusted findings, reinforcing the robustness of the conclusions.

Finally, I formally test the link between changes in liquidity commonality and liquidity risk by examining the pairwise correlations between the two across measures. Using the three liquidity measures ($QSPR$, $PQSPR$, and $Amihud$), I compute a 4×4 correlation matrix of the changes from Window 1 to Window 2.

Table 3.9 reveals two key insights. First, changes in liquidity commonality across different liquidity measures are strongly and positively correlated, indicating that firms exhibiting reductions in one measure tend to experience reductions across others. Second, and more critically, changes in liquidity commonality are significantly correlated with changes in liquidity risk. On average, when firms experience a decline in liquidity comovement, they also experience a decline in their exposure to systematic liquidity risk—consistent with the theoretical framework of Acharya and Pedersen (2005), in which comovement between firm and market liquidity is a central form of priced liquidity risk.

3.6 Robustness

3.6.1 Non-Open Market Repurchase Programs

The earlier analysis showed that firms engaged in open market repurchase (OMR) programs experience a significant and temporary reduction in liquidity commonality, with levels rebounding to their pre-repurchase values once the program concludes. This pattern was interpreted as evidence that firms' trading activity during OMR programs acts as a stabilizing force, reducing exposure to systematic liquidity shocks.

If this mechanism indeed operates through firms' active trading in open market programs, I should not expect to observe similar dynamics in other forms of share repurchase, such as tender offers or Dutch auctions, where firms repurchase shares in bulk without participating in the open market over time.

To test this prediction, I collect non-OMR repurchase programs from the SDC Mergers and Acquisitions database, applying the same filtering criteria described in Section 3.2, but restricting the sample to transactions explicitly labeled as non-open market. This yields a sample of 360 non-OMR programs. I then apply the same methodology used for the OMR analysis, estimating changes in liquidity commonality across the three event windows.

Table 3.10 reports the results. Panel A presents changes in liquidity commonality from Window 1 to Window 2, and Panel B reports changes from Window 2 to Window 3. The key finding is that, unlike OMR programs, non-open market repurchases show no significant reduction in liquidity commonality during the repurchase window, nor a subsequent rebound afterward. This suggests that the patterns observed earlier are unique to OMR programs and specifically linked to firms' gradual and flexible trading activity in the open market.

3.6.2 Weekly Frequency

The main analysis in Section 3.3 was conducted using daily data. While this high-frequency approach allows for precise estimation, it raises the possibility that the observed patterns might be influenced by microstructure noise, non-synchronous trading, or lagged adjustment effects. To address this concern, I re-estimate the main liquidity commonality regressions using weekly data.

Specifically, I estimate the following simplified version of equation (3.1), excluding control variables due to the smaller number of observations per window:

$$DL_{i,w} = \alpha_i + \beta_i DL_{M,w} + \epsilon_{i,w}, \tag{3.6}$$

where $DL_{i,w}$ is the percentage change in liquidity for stock i from week $w-1$ to w, and $DL_{M,w}$ is the concurrent change in the cross-sectional average market liquidity.

As before, I estimate this equation for each firm-program observation across the three event windows and for the three liquidity measures ($QSPR$, $PQSPR$, and $Amihud$). Table 3.11 presents the results, with Panel A showing changes from Window 1 to Window 2 and Panel B showing changes from Window 2 to Window 3. The pattern closely replicates the daily-frequency findings: liquidity commonality declines significantly during the repurchase window and reverts to pre-repurchase levels afterward. This provides additional confidence that the main results are robust and not an artifact of high-frequency data, non-synchronous trading, or microstructure effects.

3.7 Summary and Conclusion

While the motives behind firms' decisions to announce and complete open market repurchase (OMR) programs have been studied extensively, less is known about the microstructural effects of firms' own trading activity during these programs. In particular, prior work has largely focused on how OMRs affect firms' own liquidity levels but has not explored how active repurchasing alters firms' exposure to systematic liquidity shocks or their liquidity risk. This paper seeks to fill that gap. To the best of my knowledge, it is the first study to examine how firms' systematic liquidity comovement and liquidity risk evolve during OMR programs.

The results are consistent with the view that firms act as buyers of last resort, dampening the effects of variation in liquidity demand from institutional investors and liquidity supply from market makers. Specifically, I find that firms experience a significant reduction in liquidity commonality following the initiation of OMR programs. This reduction is temporary, with liquidity commonality reverting to pre-repurchase levels after the program concludes. Importantly, the decline is concentrated on days with negative market order imbalance, underscoring the asymmetric nature of firms' stabilizing role. Further analyses show that the reduction in liquidity commonality is related to both demand-side and supply-side channels, indicating that firms absorb shocks from institutional flows as well as market maker constraints. Finally, I show that the reduction in firms' liquidity commonality is accompanied by a meaningful, though temporary, decline in their exposure to liquidity risk.

Together, these findings highlight an underexplored but important dimension of OMR programs: the role of firms' trading activity in shaping the liquidity dynamics and risk profile of their own shares. By acting as stabilizing agents during periods of systematic liquidity stress, repurchasing firms not only influence their own market microstructure conditions but also reduce their exposure to priced sources of risk, with potential implications for asset pricing and market stability.

3.8 Tables

Table 3.1. Summary Statistics

This table reports summary statistics for 1,095 open market share repurchase (OMR) programs announced by U.S. firms between April 9, 1993, and October 8, 2019, from the SDC Mergers and Acquisitions database. For each variable, the mean is reported on the first line and the median is in parentheses beneath. The sample is constructed following the filters detailed in Section 3.2, which restricts to completed OMR programs for common stock on major U.S. exchanges, with no overlapping repurchase activity. *Size* is the firm's market capitalization decile based on NYSE breakpoints, and *BM* is the book-to-market ratio. *Duration* is the number of calendar days from program initiation to completion. *ProgramSize* is the announced repurchase size as a percentage of shares outstanding, and *Rep* is the percentage of *ProgramSize* that was actually repurchased by the firm.

Period	*N*	*Size*	*BM*	*Duration*	*ProgramSize(%)*	*Rep(%)*
1993–1999	487	7.8	1.6	450	6.6	98.2
		(7)	(1.4)	(278)	(5.0)	(90.0)
2000–2009	365	7.9	2.3	462	9.7	121.0
		(7)	(1.6)	(318)	(4.8)	(100.0)
2010–2019	243	7.2	1.9	295	7.0	98.8
		(7)	(1.4)	(227)	(5.2)	(99.6)
All	1,095	7.7	1.9	404	7.7	102.0
		(7)	(1.5)	(283)	(5.0)	(100.0)

Table 3.2. Changes in Liquidity Commonality

This table reports changes in the liquidity commonality coefficients of repurchasing firms across three event windows surrounding open market repurchase (OMR) programs. For each firm-program observation, I estimate equation (3.1) using three liquidity measures: dollar quoted bid-ask spread ($QSPR$), percentage quoted bid-ask spread ($PQSPR$), and the Amihud illiquidity measure ($Amihud$) as defined in Amihud (2002). Window 1 spans the six months prior to the OMR announcement, Window 2 covers the six months following the announcement, and Window 3 begins one year after the announcement and extends for six months. Panel A reports mean and median changes in liquidity commonality from Window 1 to Window 2; Panel B reports changes from Window 2 to Window 3. Columns (4) and (5) express these changes as percentages of the mean or median coefficient in Window 1. Column (6) shows the fraction of observations exhibiting a reduction or increase. P-values for the mean and median changes are reported in parentheses, based on two-tailed t-tests and Wilcoxon signed-rank tests, respectively.

	Mean	Median	Mean(%)	Median(%)	Fraction (+) or (-)
Panel A: Between window #1 & #2					
$\Delta\beta_{QSPR}$	-0.176	-0.092	-14.6%	-11.3%	59.2% (-)
	(0.01)	(0.00)			
$\Delta\beta_{PQSPR}$	-0.145	-0.076	-14.9%	-10.7%	61.3% (-)
	(0.02)	(0.00)			
$\Delta\beta_{Amihud}$	-0.163	-0.086	-15.2%	-11.4%	60.0% (-)
	(0.01)	(0.00)			
Panel B: Between window #2 & #3					
$\Delta\beta_{QSPR}$	0.181	0.095	15.0%	11.6%	56.8% (+)
	(0.03)	(0.00)			
$\Delta\beta_{PQSPR}$	0.150	0.081	15.4%	11.4%	58.1% (+)
	(0.01)	(0.00)			
$\Delta\beta_{Amihud}$	0.159	0.084	14.8%	11.3%	59.2% (+)
	(0.02)	(0.00)			

Number of Obs=1,095

Table 3.3. Adjusted Changes in Liquidity Commonality

This table reports changes in liquidity commonality coefficients across three estimation windows, adjusted to account for firm-, industry-, and time-specific effects. For each repurchasing firm, I identify a matched non-repurchasing firm from the same industry and size decile, with the closest book-to-market ratio, and no active repurchase program during the estimation period. The adjusted change is computed as the difference between the repurchasing firm's change and its matched firm's change: $\Delta\beta_i^{\text{adjusted}} = \Delta\beta_i - \Delta\beta_{\text{matched}}$. Panel A reports mean and median adjusted changes in commonality from Window 1 (pre-repurchase) to Window 2 (repurchase period); Panel B reports adjusted changes from Window 2 to Window 3 (post-repurchase). Columns (4) and (5) express these changes as percentages relative to the mean or median coefficient in Window 1. Column (6) reports the fraction of firms showing reductions (–) or increases (+) in commonality. P-values for the mean and median (columns 2 and 3) are based on two-tailed t-tests and Wilcoxon signed-rank tests, respectively.

	Mean	Median	Mean(%)	Median(%)	Fraction (+) or (-)
Panel A: Between window #1 & #2					
$\Delta\beta_{QSPR}$	-0.185	-0.097	-15.3%	-11.9%	59.7% (-)
	(0.01)	(0.00)			
$\Delta\beta_{PQSPR}$	-0.150	-0.081	-15.4%	-11.4%	61.5% (-)
	(0.01)	(0.00)			
$\Delta\beta_{Amihud}$	-0.172	-0.094	-16.0%	-12.4%	60.7% (-)
	(0.00)	(0.00)			
Panel B: Between window #2 & #3					
$\Delta\beta_{QSPR}$	0.172	0.089	14.2%	10.8%	56.4% (+)
	(0.03)	(0.00)			
$\Delta\beta_{PQSPR}$	0.142	0.074	14.7%	10.4%	57.8% (+)
	(0.02)	(0.00)			
$\Delta\beta_{Amihud}$	0.148	0.079	13.7%	10.6%	58.8% (+)
	(0.02)	(0.00)			

Number of Obs=1,095

Table 3.4. Changes in Signed Liquidity Commonality Coefficients

This table reports changes in liquidity commonality coefficients separately for days with positive and negative market order imbalance, estimated using $QSPR$ (dollar quoted bid-ask spread) as the liquidity measure. For each firm-program observation, equation (3.3) is estimated over three windows: Window 1 (pre-repurchase), Window 2 (repurchase period), and Window 3 (post-repurchase). β_i^p denotes the commonality coefficient on days of positive market order imbalance (net buying), while β_i^n denotes the coefficient on days of negative market order imbalance (net selling). Panel A reports the mean and median changes from Window 1 to Window 2; Panel B reports changes from Window 2 to Window 3. Columns (4) and (5) present changes as percentages relative to the Window 1 baseline. Column (6) shows the fraction of firms with reductions (–) or increases (+) in commonality. P-values (in parentheses) are based on two-tailed t-tests (mean) and Wilcoxon signed-rank tests (median).

	Mean	Median	Mean(%)	Median (%)	Fraction (+) or (-)
Panel A: Between window #1 & #2					
$\Delta\beta^n$	-0.382	-0.215	-33.7%	-28.4%	67.3% (-)
	(0.00)	(0.00)			
$\Delta\beta^p$	-0.075	0.005	-6.9%	0.6%	50.1% (+)
	(0.15)	(0.82)			
Panel B: Between window #2 & #3					
$\Delta\beta^n$	+0.335	+0.152	29.5%	20.0%	63.7% (+)
	(0.00)	(0.00)			
$\Delta\beta^p$	0.062	0.056	5.7%	6.7%	50.9% (+)
	(0.23)	(0.27)			

Number of Obs=1,095

Table 3.5. Relation Between Institutional Ownership and Liquidity Commonality

This table reports the results of six univariate regressions examining the relationship between institutional ownership and liquidity commonality. The explanatory variable, $inst$, is defined as the percentage of firm shares held by institutional investors in the quarter prior to the OMR announcement, sourced from the CDA/Spectrum database. Panel A reports regressions where the dependent variable is β_i^p, the liquidity commonality coefficient on days of positive market order imbalance (net buying), estimated separately for Window 1 (pre-repurchase), Window 2 (repurchase period), and Window 3 (post-repurchase). Panel B reports regressions where the dependent variable is β_i^n, the liquidity commonality coefficient on days of negative market order imbalance (net selling), across the same three windows. Reported coefficients show the estimated relation between institutional ownership and commonality; R^2 values indicate the explanatory power of institutional ownership in each regression. P-values are shown in parentheses.

Panel A:

	$\beta^{p,window1}$	$\beta^{p,window2}$	$\beta^{p,window3}$
$inst$	0.96	1.09	1.07
	(0.00)	(0.00)	(0.00)
R^2	0.11	0.10	0.12

Panel B:

	$\beta^{n,window1}$	$\beta^{n,window2}$	$\beta^{n,window3}$
$inst$	1.67	0.14	1.45
	(0.00)	(0.71)	(0.01)
R^2	0.08	0.00	0.07
N	1,095	1,095	1,095

Table 3.6. Effects of Market Volatility and Short-Term Interest Rate Shocks on Liquidity Commonality

This table reports the estimation results of equation (3.4), which examines how liquidity commonality responds to supply-side shocks. For each firm-program observation and estimation window, I estimate the firm-level coefficients t_i and s_i, which capture the excess sensitivity of liquidity commonality to short-term funding shocks (TED spread residuals) and market volatility shocks, respectively. Specifically, t_i measures the additional comovement in firm i's liquidity with the market on days of positive TED spread shocks (tight funding conditions), while s_i captures excess liquidity comovement on days of elevated market volatility. Daily TED spread shocks are computed as residuals from an autoregressive model of the TED spread, and daily volatility shocks are calculated following the residual-based procedure of Schwert (1990). The table reports the mean and median values of t_i and s_i across firms in each of the three event windows: Window 1 (pre-repurchase), Window 2 (repurchase period), and Window 3 (post-repurchase). P-values in parentheses are based on two-tailed t-tests (means) and Wilcoxon signed-rank tests (medians).

	$t_i(TED)$		$s_i(Volatility)$	
	Mean	Median	Mean	Median
Window 1:				
	2.40	0.99	2.16	1.25
	(0.00)	(0.00)	(0.03)	(0.00)
Window 2:				
	-0.33	0.25	-0.04	0.24
	(0.33)	(0.73)	(0.97)	(0.39)
Window 3:				
	1.57	0.42	1.08	1.43
	(0.06)	(0.07)	(0.08)	(0.07)

Number of Obs=1,095

Table 3.7. Changes in Risk Betas Across Estimation Windows

This table reports the changes in risk factor loadings (betas) for firms undergoing open market repurchase (OMR) programs, estimated using the five-factor asset pricing model. The model includes the standard Fama-French-Carhart factors—market (β^{mkt}), size (β^{smb}), value (β^{hml}), and momentum (β^{mom})—as well as a tradable liquidity risk factor (β^{ps}) constructed following Pástor and Stambaugh (2003), Li et al. (2019), and Pontiff and Singla (2019). For each firm-program observation, I estimate betas separately over three windows: Window 1 (pre-repurchase, six months prior to announcement), Window 2 (repurchase period, six months post-announcement), and Window 3 (post-repurchase, starting one year after announcement). Panel A reports the mean and median changes in betas from Window 1 to Window 2; Panel B reports changes from Window 2 to Window 3. The final column shows the fraction of firms with positive (+) or negative (–) changes. P-values in parentheses are based on two-tailed t-tests (mean) and Wilcoxon rank tests (median).

	Mean	Median	Fraction (+) or (-)
Panel A: Between window #1 & #2			
$\Delta\beta^{mkt}$	-0.02	0.02	50.7% (+)
	(0.13)	(0.63)	
$\Delta\beta^{smb}$	-0.04	-0.04	54.3% (-)
	(0.02)	(0.00)	
$\Delta\beta^{hml}$	0.01	-0.02	50.9% (-)
	(0.17)	(0.20)	
$\Delta\beta^{mom}$	-0.01	-0.02	50.8% (-)
	(0.67)	(0.24)	
$\Delta\beta^{ps}$	-0.06	-0.07	62.3% (-)
	(0.00)	(0.00)	
Panel B: Between window #2 & #3			
$\Delta\beta^{mkt}$	0.01	0.01	50.8% (+)
	(0.54)	(0.78)	
$\Delta\beta^{smb}$	0.02	0.01	51.0% (+)
	(0.28)	(0.41)	
$\Delta\beta^{hml}$	-0.01	-0.01	50.3% (-)
	(0.76)	(0.81)	
$\Delta\beta^{mom}$	0.01	-0.01	50.4% (-)
	(0.63)	(0.61)	
$\Delta\beta^{ps}$	0.06	0.06	60.3% (+)
	(0.00)	(0.00)	

Number of Obs=1,095

Table 3.8. Adjusted Changes in Risk Betas Across Estimation Windows

This table reports the adjusted changes in risk factor loadings (betas) for firms undergoing open market repurchase (OMR) programs. To control for industry-time fixed effects, each repurchasing firm is matched to a non-repurchasing firm from the same industry and size decile (selected based on the closest book-to-market ratio), as described in Section 3.1. Adjusted changes are computed as the difference between the change in the repurchasing firm's beta and the corresponding change in its matched control: $\Delta\beta_i^{\text{adjusted}} = \Delta\beta_i - \Delta\beta_{\text{matched}}$. The asset pricing model (equation 4) includes market (β^{mkt}), size (β^{smb}), value (β^{hml}), momentum (β^{mom}), and tradable liquidity risk (β^{ps}) factors, estimated over three event windows: Window 1 (pre-repurchase), Window 2 (repurchase period), and Window 3 (post-repurchase). Panel A reports adjusted mean and median beta changes from Window 1 to Window 2; Panel B reports adjusted changes from Window 2 to Window 3. The final column shows the fraction of firms with positive (+) or negative (–) changes. P-values are reported in parentheses, based on two-tailed t-tests (mean) and Wilcoxon rank tests (median).

	Mean	Median	Fraction (+) or (-)
Panel A: Between window #1 & #2			
$\Delta\beta^{mkt}$	-0.01	0.02	50.5% (+)
	(0.37)	(0.59)	
$\Delta\beta^{smb}$	-0.04	-0.05	54.7% (-)
	(0.01)	(0.00)	
$\Delta\beta^{hml}$	-0.01	-0.02	50.9% (-)
	(0.24)	(0.27)	
$\Delta\beta^{mom}$	-0.02	0.00	50.1% (+)
	(0.71)	(0.81)	
$\Delta\beta^{ps}$	-0.07	-0.07	62.4% (-)
	(0.00)	(0.00)	
Panel B: Between window #2 & #3			
$\Delta\beta^{mkt}$	0.01	0.00	50.1% (+)
	(0.71)	(0.88)	
$\Delta\beta^{smb}$	0.01	0.01	50.9% (+)
	(0.25)	(0.39)	
$\Delta\beta^{hml}$	0.00	-0.01	50.4% (-)
	(0.17)	(0.20)	
$\Delta\beta^{mom}$	0.01	0.01	50.8% (+)
	(0.65)	(0.31)	
$\Delta\beta^{ps}$	0.07	0.06	60.6% (+)
	(0.00)	(0.00)	

Number of Obs=1,095

Table 3.9. Correlation matrix between changes in liquidity commonality coefficients and changes in liquidity risk beta

This table reports the pairwise correlation matrix between changes in liquidity commonality coefficients and changes in liquidity risk beta from Window 1 (pre-repurchase) to Window 2 (repurchase period). $\Delta\beta_{QSPR}$, $\Delta\beta_{PQSPR}$, and $\Delta\beta_{Amihud}$ represent the changes in liquidity commonality coefficients based on the dollar quoted bid-ask spread ($QSPR$), percentage quoted bid-ask spread ($PQSPR$), and Amihud illiquidity measure, respectively. $\Delta\beta^{ps}$ denotes the change in the liquidity risk beta estimated from the Pastor-Stambaugh liquidity factor in the five-factor asset pricing model. Correlations are computed across the full sample of 1,095 firm-program observations. Numbers in parentheses are p-values testing the null hypothesis of zero correlation.

	$\Delta\beta_{QSPR}$	$\Delta\beta_{PQSPR}$	$\Delta\beta_{Amihud}$	ΔPS
$\Delta\beta_{QSPR}$	1			
$\Delta\beta_{PQSPR}$	0.91	1		
	(0.00)			
$\Delta\beta_{Amihud}$	0.82	0.89	1	
	(0.00)	(0.00)		
$\Delta\beta^{ps}$	0.26	0.32	0.30	1
	(0.03)	(0.01)	(0.01)	

Number of Obs=1,095

Table 3.10. Changes in liquidity commonality, non-open market repurchase programs

This table reports changes in liquidity commonality coefficients between three estimation windows for non-open market repurchase programs (non-OMRs), including tender offers and Dutch auctions. For each liquidity measure—dollar quoted bid-ask spread ($QSPR$), percentage quoted bid-ask spread ($PQSPR$), and Amihud illiquidity ($Amihud$)—I estimate equation (1) separately over Window 1 (six months pre-announcement), Window 2 (six months post-announcement), and Window 3 (months 12–18 post-announcement). Panel A presents the mean and median change in liquidity commonality from Window 1 to Window 2; Panel B reports changes from Window 2 to Window 3. Columns (4) and (5) express changes as percentages of the mean and median levels in Window 1, and column (6) reports the fraction of observations showing a reduction (-) or increase (+). P-values in parentheses are based on two-tailed t-tests (means) and Wilcoxon signed-rank tests (medians). The sample consists of 360 non-OMR repurchase programs drawn from the SDC Mergers and Acquisitions database.

	Mean	Median	Mean(%)	Median(%)	Fraction (+) or (-)
Panel A: Between window #1 & #2					
$\Delta\beta_{QSPR}$	0.050	0.030	3.3%	3.4%	50.5% (+)
	(0.20)	(0.60)			
$\Delta\beta_{PQSPR}$	0.068	-0.042	5.4%	-5.3%	50.1% (-)
	(0.56)	(0.90)			
$\Delta\beta_{Amihud}$	0.051	-0.033	3.9%	-4.0%	50.7% (-)
	(0.66)	(0.47)			
Panel B: Between window #2 & #3					
$\Delta\beta_{QSPR}$	0.042	0.031	2.8%	3.5%	50.4% (+)
	(0.62)	(0.53)			
$\Delta\beta_{PQSPR}$	0.028	0.016	2.2%	2.0%	50.6% (+)
	(0.82)	(0.41)			
$\Delta\beta_{Amihud}$	0.031	0.029	2.3%	3.5%	50.9% (+)
	(0.88)	(0.61)			

Number of Obs=360

Table 3.11. Changes in liquidity commonality, weekly frequency

This table reports changes in liquidity commonality coefficients between three estimation windows, using weekly rather than daily observations. For each firm-program observation, I estimate equation (3.6) over Window 1 (six months prior to repurchase announcement), Window 2 (six months following the announcement), and Window 3 (months 12–18 post-announcement), focusing on three liquidity measures: dollar quoted bid-ask spread ($QSPR$), percentage quoted bid-ask spread ($PQSPR$), and Amihud illiquidity ($Amihud$). To account for the reduced number of weekly observations, regressions exclude control variables. Panel A reports the mean and median changes in liquidity commonality from Window 1 to Window 2; Panel B reports changes from Window 2 to Window 3. Columns (4) and (5) present changes as percentages of the mean and median levels in Window 1, while column (6) shows the percentage of observations with reductions (-) or increases (+). P-values for means and medians are based on two-tailed t-tests and Wilcoxon signed-rank tests, respectively.

	Mean	Median	Mean(%)	Median(%)	Fraction (+) or (-)
Panel A: Between window #1 & #2					
$\Delta\beta_{QSPR}$	-0.115	-0.072	-11.8%	-8.2%	59.5% (-)
	(0.03)	(0.00)			
$\Delta\beta_{PQSPR}$	-0.105	-0.065	-10.5%	-6.6%	60.7% (-)
	(0.03)	(0.00)			
$\Delta\beta_{Amihud}$	-0.121	-0.071	-11.5%	-7.4%	60.5% (-)
	(0.02)	(0.00)			
Panel B: Between window #2 & #3					
$\Delta\beta_{QSPR}$	0.121	0.075	12.4%	8.6%	58.8% (+)
	(0.03)	(0.00)			
$\Delta\beta_{PQSPR}$	0.110	0.071	11.0%	7.2%	58.4% (+)
	(0.04)	(0.00)			
$\Delta\beta_{Amihud}$	0.132	0.084	12.5%	8.8%	58.9% (+)
	(0.01)	(0.00)			

Number of Obs=1,095

Bibliography

Acharya, V. V. and Pedersen, L. H. (2005). Asset pricing with liquidity risk. *Journal of financial Economics*, 77(2):375–410.

Akhigbe, A., Kim, D., and Madura, J. (2007). Price performance following share-repurchase announcements by closed-end funds. *Financial Review*, 42(4):537–555.

Amihud, Y. (2002). Illiquidity and stock returns: cross-section and time-series effects. *Journal of financial markets*, 5(1):31–56.

Anolick, N., Batten, J. A., Kinateder, H., and Wagner, N. (2021). Time for gift giving: Abnormal share repurchase returns and uncertainty. *Journal of Corporate Finance*, 66:101787.

Anton, M. and Polk, C. (2014). Connected stocks. *The Journal of Finance*, 69(3):1099–1127.

Baker, M., Stein, J. C., and Wurgler, J. (2003). When does the market matter? stock prices and the investment of equity-dependent firms. *The quarterly journal of economics*, 118(3):969–1005.

Banyi, M. L., Dyl, E. A., and Kahle, K. M. (2008). Errors in estimating share repurchases. *Journal of Corporate Finance*, 14(4):460–474.

Barclay, M. J. and Smith, C. W. (1988). Corporate payout policy: Cash dividends versus open-market repurchases. *Journal of Financial Economics*, 22(1):61–82.

Bartov, E. (1991). Open-market stock repurchases as signals for earnings and risk changes. *Journal of Accounting and Economics*, 14(3):275–294.

Bhattacharya, U. and Jacobsen, S. (2016). The share repurchase announcement puzzle: Theory and evidence. *Review of Finance*, 20(2):725–758.

Bonaimé, A. A. (2015). Mandatory disclosure and firm behavior: Evidence from share repurchases. *The Accounting Review*, 90(4):1333–1362.

Brav, A., Graham, J. R., Harvey, C. R., and Michaely, R. (2005). Payout policy in the 21st century. *Journal of financial economics*, 77(3):483–527.

Brunnermeier, M. K. and Pedersen, L. H. (2009). Market liquidity and funding liquidity. *The review of financial studies*, 22(6):2201–2238.

Busch, P. and Obernberger, S. (2017). Actual share repurchases, price efficiency, and the information content of stock prices. *The review of financial studies*, 30(1):324–362.

Chan, K., Ikenberry, D. L., Lee, I., and Wang, Y. (2010). Share repurchases as a potential tool to mislead investors. *Journal of Corporate Finance*, 16(2):137–158.

Chevalier, J. and Ellison, G. (1997). Risk taking by mutual funds as a response to incentives. *Journal of political economy*, 105(6):1167–1200.

Chiu, H.-H. and Kini, O. (2014). Equity issuances, equity mutual fund flows, and noise trader sentiment. *Review of Finance*, 18(2):749–802.

Chordia, T., Roll, R., and Subrahmanyam, A. (2000). Commonality in liquidity. *Journal of financial economics*, 56(1):3–28.

Comment, R. and Jarrell, G. A. (1991). The relative signalling power of dutch-auction and fixed-price self-tender offers and open-market share repurchases. *The Journal of Finance*, 46(4):1243–1271.

Coughenour, J. F. and Saad, M. M. (2004). Common market makers and commonality in liquidity. *Journal of Financial economics*, 73(1):37–69.

Coval, J. and Stafford, E. (2007). Asset fire sales (and purchases) in equity markets. *Journal of Financial Economics*, 86(2):479–512.

Crane, A. D., Michenaud, S., and Weston, J. P. (2016). The effect of institutional ownership on payout policy: Evidence from index thresholds. *The Review of Financial Studies*, 29(6):1377–1408.

Dudley, E. and Manakyan, A. (2011). Corporate repurchase decisions following mutual fund sales. *Financial Management*, 40(4):973–999.

Edmans, A., Goldstein, I., and Jiang, W. (2012). The real effects of financial markets: The impact of prices on takeovers. *The Journal of Finance*, 67(3):933–971.

Ellul, A., Jotikasthira, C., and Lundblad, C. T. (2011). Regulatory pressure and fire sales in the corporate bond market. *Journal of Financial Economics*, 101(3):596–620.

Fama, E. F. and French, K. R. (1993). Common risk factors in the returns on stocks and bonds. *Journal of financial economics*, 33(1):3–56.

Fama, E. F. and MacBeth, J. D. (1973). Risk, return, and equilibrium: Empirical tests. *Journal of political economy*, 81(3):607–636.

Franz, D. R., Rao, R. P., and Tripathy, N. (1995). Informed trading risk and bid-ask spread changes around open market stock repurchases in the nasdaq market. *Journal of Financial Research*, 18(3):311–327.

Grinstein, Y. and Michaely, R. (2005). Institutional holdings and payout policy. *The Journal of Finance*, 60(3):1389–1426.

Grullon, G. and Michaely, R. (2004). The information content of share repurchase programs. *The Journal of Finance*, 59(2):651–680.

Hameed, A., Kang, W., and Viswanathan, S. (2010). Stock market declines and liquidity. *The Journal of finance*, 65(1):257–293.

Hasbrouck, J. and Seppi, D. J. (2001). Common factors in prices, order flows, and liquidity. *Journal of financial Economics*, 59(3):383–411.

Hau, H. and Lai, S. (2013). Real effects of stock underpricing. *Journal of Financial Economics*, 108(2):392–408.

Hillert, A., Maug, E., and Obernberger, S. (2016). Stock repurchases and liquidity. *Journal of financial economics*, 119(1):186–209.

Hong, H., Wang, J., and Yu, J. (2008). Firms as buyers of last resort. *Journal of Financial Economics*, 88(1):119–145.

Huberman, G. and Halka, D. (2001). Systematic liquidity. *Journal of Financial Research*, 24(2):161–178.

Ikenberry, D., Lakonishok, J., and Vermaelen, T. (1995). Market underreaction to open market share repurchases. *Journal of financial economics*, 39(2-3):181–208.

Ikenberry, D., Lakonishok, J., and Vermaelen, T. (2000). Stock repurchases in canada: Performance and strategic trading. *The Journal of Finance*, 55(5):2373–2397.

Kahle, K. M. (2002). When a buyback isn't a buyback: Open market repurchases and employee options. *Journal of Financial Economics*, 63(2):235–261.

Kamara, A., Lou, X., and Sadka, R. (2008). The divergence of liquidity commonality in the cross-section of stocks. *Journal of Financial Economics*, 89(3):444–466.

Karolyi, G. A., Lee, K.-H., and Van Dijk, M. A. (2012). Understanding commonality in liquidity around the world. *Journal of financial economics*, 105(1):82–112.

Khan, M., Kogan, L., and Serafeim, G. (2012). Mutual fund trading pressure: Firm-level stock price impact and timing of seos. *The Journal of Finance*, 67(4):1371–1395.

Kisin, R. (2011). The impact of mutual fund ownership on corporate investment: Evidence from a natural experiment. *Available at SSRN 1828183*.

Koch, A., Ruenzi, S., and Starks, L. (2016). Commonality in liquidity: a demand-side explanation. *The Review of Financial Studies*, 29(8):1943–1974.

Korajczyk, R. A. and Sadka, R. (2008). Pricing the commonality across alternative measures of liquidity. *Journal of Financial Economics*, 87(1):45–72.

Lakonishok, J. and Vermaelen, T. (1990). Anomalous price behavior around repurchase tender offers. *The Journal of Finance*, 45(2):455–477.

Leng, F. and Noronha, G. (2013). Information and long-term stock performance following open-market share repurchases. *Financial Review*, 48(3):461–487.

Li, H., Novy-Marx, R., Velikov, M., et al. (2019). Liquidity risk and asset pricing. *Critical Finance Review*, 8(1-2):223–255.

Lie, E. (2005). Operating performance following open market share repurchase announcements. *Journal of Accounting and Economics*, 39(3):411–436.

Lou, D. (2012). A flow-based explanation for return predictability. *The Review of Financial Studies*, 25(12):3457–3489.

Mitchell, M., Pedersen, L. H., and Pulvino, T. (2007). Slow moving capital. *American Economic Review*, 97(2):215–220.

Nayar, N., Singh, A. K., and Zebedee, A. A. (2008). Share repurchase offers and liquidity: An examination of temporary and permanent effects. *Financial Management*, 37(2):251–270.

Newey, W. K. and West, K. D. (1987). A simple, positive semi-definite, heteroskedasticity and autocorrelation consistent covariance matrix. *Econometrica*, 55(3):703–708.

Pástor, L. and Stambaugh, R. F. (2003). Liquidity risk and expected stock returns. *Journal of Political economy*, 111(3):642–685.

Peyer, U. and Vermaelen, T. (2009). The nature and persistence of buyback anomalies. *The Review of Financial Studies*, 22(4):1693–1745.

Pontiff, J. and Singla, R. (2019). Liquidity risk? *Available at SSRN 3320011*.

Pulvino, T. C. (1998). Do asset fire sales exist? an empirical investigation of commercial aircraft transactions. *The Journal of Finance*, 53(3):939–978.

Sadka, R. (2006). Momentum and post-earnings-announcement drift anomalies: The role of liquidity risk. *Journal of Financial Economics*, 80(2):309–349.

Schwert, G. W. (1990). Stock volatility and the crash of'87. *The review of financial studies*, 3(1):77–102.

Sirri, E. R. and Tufano, P. (1998). Costly search and mutual fund flows. *The journal of finance*, 53(5):1589–1622.

Stephens, C. P. and Weisbach, M. S. (1998). Actual share reacquisitions in open-market repurchase programs. *The Journal of finance*, 53(1):313–333.

Vermaelen, T. (1981). Common stock repurchases and market signalling: An empirical study. *Journal of financial economics*, 9(2):139–183.

Wang, C.-S., Strong, N., Tung, S., and Lin, S. (2009). Share repurchases, the clustering problem, and the free cash flow hypothesis. *Financial Management*, 38(3):487–505.

Wiggins, J. B. (1994). Open market stock repurchase programs and liquidity. *Journal of Financial Research*, 17(2):217–229.

Yook, K. C. (2010). Long-run stock performance following stock repurchases. *The Quarterly Review of Economics and Finance*, 50(3):323–331.